SCIENCE

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THE STUDY OF HUMAN BEHAVIOR[1]

My part in this symposium must be that of the comparative psychologist who, while professionally engaged in the experimental study of the behavior of lower organisms, is keenly interested in human behavior and in the development of methods by which it may be profitably studied. I propose, in this discussion, to try to bring some of the experiences of the student of the behavior of animals to bear upon the problems which the eugenic investigator meets. Especially I shall attempt to indicate the necessity for an analytic procedure on the basis of carefully wrought out methods of experimental control and observation, and the thorough-going study of the components of human responses to situations rather than of complexes such as are actually presented to us in the behavior of human beings.

Human behavior is only a part, albeit a most important part, of the materials of the general science of organic behavior. It presents essentially the same kinds of problems as does the behavior of any other mammal; and it must be studied by methods similar to, if not actually identical with, those employed by the student of infra-human behavior. I should be in-

[1] The substance of the writer's contribution to a symposium on the Study of Human Behavior at the Conference on Eugenics, held at Cold Spring Harbor, Long Island, June 19 and 20, 1913. Being Contributions from the Psychopathic Hospital, Boston, Massachusetts, No. 25 (1913. 25): previous Contribution, P. H. Contributions 1913. 24, was Robert M. Yerkes: "Comparative Psychology: A Question of Definitions." *The Journal of Philosophy, Psychology and Scientific Methods,* October 9, 1913.

clined to go still further in maintaining that a student of comparative psychology, or the behaviorist in the wide sense, is admirably fitted, by his experience in dealing with varied forms of behavior, by his knowledge of the genetic relations or developmental history of organic behavior, and by the ingenuity in devising and using experimental methods which his work forces him to acquire, to formulate problems in human behavior and to suggest methods of solving them.

There are at least three valuable points of view from which the behavior of man may be studied: the psychological; the physiological; and the pragmatological.[2] The first inclines the observer to an analytic study of activity in its relations to the ideas, motives, purposes and ideals of the organism; in a word, to a study whose goal is the description of behavior as an expression of will. The second disposes the investigator to seek definite knowledge of the functions of motor organs and of the relations of those mechanisms to other bodily organs and processes. From this point of view, the organism is studied as a living thing, influenced by environment and reacting upon it. The third directs him to the products of organic activity, as keys to the meaning of conscious behavior. It may reasonably be maintained that the understanding of the behavior of any organism, and most of all that of man, is conditioned by reliable and reasonably complete knowledge of the experiences of the organism, of its life processes, including its complex as well as its simple forms of behavior, and of the products or results of its bodily activities. We may consider fortunate those students of human behavior who are able to take, in turn, as occasion demands or as opportunity makes possible, these three points of view, for we must recognize that man is a self-conscious being who exhibits varied and complex activities and at the same time produces works of science, industry and art which are of deep psychological significance. This, however, is merely equivalent to the statement that human behavior may be clearly understood only if viewed in its relations, on the one hand, to its structural conditions, and on the other, to its purposes and results.

The history of science indicates that man has been surprisingly slow to come into close quarters, in a strictly scientific manner, with his own behavior. Of psychological, neurological, ethnological and archeological facts we to-day have an abundance. But we know relatively little concerning the facts and laws of human activity. This is true, I believe, chiefly for two reasons. First, because consciousness, presenting as it does a perennially puzzling problem in its relations to body, has absorbed attention; and, second, because the results of human behavior have proved more engaging to most observers, even to most scientific observers, than the behavior itself. To-day, a change is evident, for here and there "human behavior" has become an object of scientific interest. Sometimes this interest is inspired by practical problems; sometimes, by the desire for scientific knowledge. Recently, there have appeared several books[3] which, although dealing with

[2] We lack a term to designate the point of view of the scientific student of the results or products of activity, such, for example, as the abodes of animals or of man, works of art, scientific inventions, etc. I have designated this point of view as pragmatological, in the absence of a more suitable term.

[3] Among these may be mentioned "The Science of Human Behavior: Biological and Psychological Foundations," by Maurice Parmelee, New York, 1913; "The Fundamental Laws of Human Behavior; lectures on the foundations of any mental or social science," by Max Meyer, Boston, 1911; "Introduction to Social Psychology," by William McDougall, London, 1908.

the general subject of man's behavior, are indicative rather of the realization of the need of knowledge than of the existence of such knowledge. Without exception, such general discussions as the writer is familiar with display our ignorance of the facts of behavior and of the principles underlying them and serve rather as arguments in favor of the systematic study of this group of phenomena than as satisfactory treatises.

The behaviorist, whether he be physiologist or psychologist at heart, seeks, first of all, accurate knowledge of the facts of behavior. His task it is to analyze behavior-complexes, to discover their conditions or causes, to formulate the laws of their appearance, and to point the way to their control. It matters not to him whether his subject happens to be a horse, an ape, or a man. He adapts his methods of investigation to the problem and the subject in hand and proceeds to gather data. It is characteristic of the recent experimental work in behavior that reactions to simple or complex situations should be broken up into smaller and smaller part processes, and the characteristics of these processes as well as their relations, studied minutely, persistently, accurately.

Our time-honored classification of activities as reflex, instinctive, impulsive, habitual, voluntary, is no longer in favor. Indeed, the speculative discussion of the characteristics of different types of activity and attempts to formulate definitions which shall render these types mutually exclusive have to-day given place to systematic searching for the characteristics or attributes of acts and groups of acts, and for genetic descriptions thereof. Only recently, it must be confessed, have we fully realized that an instinctive act is something to be observed and reobserved under varied and rigidly controlled conditions; something to be studied in its origin and development; something to be created, if possible, by the control of conditions of organic behavior, rather than something merely to be talked about or defined in abstract terms. The older literature of instinct is vague, general, speculative. The newer is definite, concrete, observational. And what is true of our studies of instinct is true, likewise, of our studies of the various senses and of habits.

It is rather late in this discussion to define behavior, yet an attempt to do so may serve to correct certain misimpressions which seem not uncommon. The term, as used by the scientist to-day, is inclusive not merely of those gross and obvious activities exhibited by man in common with the other animals, but of hidden organic processes. The behaviorist is interested quite as much in reflexes, which might ordinarily be relegated to physiology, as in habitual, or instinctive, or voluntary acts. But he is interested, also, as much in the complex forms of behavior, known as conduct, as in the simpler expressions of human intelligence. Indeed, even the most complex moral and religious forms of activity are regarded by him as material for scientific study.

A strictly scientific study of the varieties of human behavior demands, first, the formulation of problems, for advance comes slowly and uncertainly unless the investigator can definitely formulate his task. It demands, further, thorough knowledge of the methods of physiology, of psychology, of physics, and of chemistry, and ingenuity on the part of the experimenter in adapting these methods to his immediate needs. Finally, it demands familiarity with the facts of behavior in other organisms, in order that the comparative method may be made to lend abundant aid.

Noteworthy changes have occurred in the study of behavior during the past twenty

years. Then, observation was naturalistic, general: now, it is experimental, specific. Most obvious and most important of the changes is the development of methods by which the conditions of observation may be controlled and the results precisely recorded. The older work was, on the whole, crude, inconclusive, wasteful of time, leading to few safe generalizations; the newer, by contrast, is precise, carefully controlled, and tending to lead to the formulation of laws. It is, of course, desirable that we recognize that not all observation can be conducted under experimental conditions, that the naturalistic method in the study of human behavior as also in the study of animal behavior has its proper and important place. We should gladly recognize its values, while insisting upon the importance of supplementing it by the experimental study of the same phenomena.

But the practical-minded person has doubtless been asking, throughout this introductory discussion, of our topic, "How may the reactions of a person be scientifically studied? Is it possible, ordinarily, to subject a human being to such conditions of observation as are used in experiments with other animals?" A few examples from studies of human and infra-human behavior will serve as an answer to these questions.

One of the most interesting aspects of organic activity is its modifiability. We designate this as habit-formation. Now, it happens that in a great variety of organisms the formation of habits has been studied experimentally. In the case of the dancing mouse,[4] for example, I investigated the relation of rapidity of habit-formation to certain external conditions. With an apparatus so arranged that the mouse could choose as its route through the experiment box either a dark or a light passageway, I determined the number of experiences necessary in order that the animal should learn that, no matter what the spacial relations of the passageways, only the light one could safely be chosen as a way of egress, since each time the dark passageway was entered a disagreeable electric shock resulted. It was the problem of the observer to discover how quickly, under a given set of conditions, the mouse would learn always to avoid the one passageway and to seek the other. In this experiment, which was so conducted that strictly comparable results were obtained from several individuals, it was first ascertained that the less the difference in lightness of the two passageways, the longer it took the mouse to learn to choose correctly. Next, it was determined that the rapidity of learning varied with the strength of the electric shock, which was regularly given as punishment for mistakes. When the passageways differed markedly (discrimination easy), the stronger the shock the more rapid the learning. When the passageways differed slightly (discrimination difficult), beyond a certain point, increase in the strength of the shock delayed the learning process. When the passageways differed by an intermediate amount, it appeared that an intermediate strength of stimulus was most favorable to habit-formation. From these observations, it was possible to deduce the following law for the behavior of the dancing mouse: As the difficultness of visual discrimination increases, that strength of electric stimulus which is most favorable as a condition for the acquisition of a habit tends to approach the threshold.

In this investigation, we have, first, a definite problem; second, a reasonably large number of observational data (facts of be-

[4] Yerkes, Robert M. and Dodson, John D., "The Relation of Strength of Stimulus to Rapidity of Habit-formation," *Journal of Comparative Neurology and Psychology*, 1908, Vol. 18, p. 459.

havior), and, third, a law of behavior for the particular organism in question. In effect, what I did with the dancing mouse might be done with human subjects, should it seem desirable to gain definite knowledge of this aspect of habit-formation. As it happens, precisely the kind of knowledge of human behavior which such experimental methods yield is necessary, if we are ever to have a thoroughgoing science of human behavior which will enable us to deal with our fellows effectively.

Another illustration may be taken from the study of imitative activity. It is generally recognized that imitation may be studied experimentally in any organism. But up to the present most observations of this group of phenomena have been casual, and our opinions concerning the importance of imitation in any organism are quite likely to be based upon insufficient or inaccurate information. With sparrows, canaries, mice, rats, cats, dogs, monkeys, experiments have been made to determine the nature and extent of imitative reaction, and there is no obvious reason why the methods of the behaviorists should not be adapted to the study of the imitative tendency in human beings.

In order to exhibit, in its general features, a method of studying imitation experimentally, we shall consider investigations of this aspect of behavior in monkeys.[5] The observer, first of all, seeks for a number of acts or series of acts which his animals either can not learn to perform of their own initiative or learn with extreme difficulty. Such acts are most readily discovered in connection with artificially arranged situations, as, for example, in connection with puzzle or problem boxes and similar experimental devices. He then teaches one animal to perform an act and thereafter, under definitely describable and constant conditions, he permits another animal to observe the behavior of the first. Any tendency for the second animal to imitate the first, or to modify its behavior in accordance with the activities of the first, is noted. Thus, by repetition after repetition of this sort of experiment, the observer strives to get definite knowledge of the nature and extent to which the behavior of one animal influences that of its fellows. It is, of course, necessary in such an experiment to work out a method carefully and to make all observations under carefully controlled conditions. It is necessary, also, to measure as precisely as possible several aspects of the behavior of the observing individual, and thus to depend not upon general impressions, but upon records which can not be influenced by any bias on the part of the experimenter.

Such experimental studies as those of Watson and Haggerty have proved that certain monkeys imitate much less generally than is commonly supposed. And further, that they imitate seldom, if ever, in the purposive manner in which man imitates. It seems that although they influence one another markedly in their behavior, this influence is chiefly a matter of the directing of attention. The imitation of means or of ends in a voluntary and wholly conscious manner rarely appears among the mammals below man.

An illustration from actual experimental work which clearly indicates the need of analyzing behavior complexes and of dealing quantitatively with simple bits of behavior is furnished by some recent work which the writer has done with rats. It was his task to try to discover the modes of heredity of savageness and wildness in rats.

[5] Haggerty, M. E., "Imitation in Monkeys," *Journal of Comparative Neurology and Psychology*, 1909, Vol. 19, p. 337; Watson, B., "Imitation in Monkeys," *Psychological Bulletin*, 1908, Vol. 5, p. 169.

In order to accomplish this task, it was necessary to analyze savageness and wildness. This meant discovering those acts or organic processes which, taken together, mean to the observer savageness or wildness. The first result of observation was that biting, squealing, struggling to escape, or attacking the experimenter, and process of excretion, appeared as important elements of savageness. The experimenter, relying upon these elements, measured, roughly, the savageness of a large number of individuals, arranging them according to their behavior in six grades, designated 0 to 5. On the basis of this obviously crude preliminary work, certain facts indicative of the mode of transmission of savageness and wildness were ascertained.[6]

In yet other observations on rats which involved the comparison of two groups, stock individuals and closely inbred individuals, it appeared that the behavior of the two groups, in the face of certain experimentally arranged situations, differed greatly. This, upon careful observation, the experimenter was able to attribute to differences in temperament. The stock rats were rather active, energetic, quick moving, whereas the inbred animals were more stolid, slow and deliberate. In order that the reactions of these individuals in various experimental situations be properly interpreted, it is essential that the experimenter obtain knowledge of their temperamental character, such, for example, as degree of nervousness or of timidity, of savageness or wildness, quickness of response, persistence, energy and so on through the list of aspects of behavior which, looked at as a whole, might be considered the temperament of the animal. The point which I am trying to emphasize is this. If we are to work effectively, with human beings or other animals, we must analyze the concrete behavior of the organism's every-day life into simpler processes and then study these processes, one by one, by means of methods which shall enable us to measure them fairly accurately and describe them with corresponding accuracy and precision.

The application of these observations in the work of the eugenics investigator are obvious, for the latter, in dealing with human behavior, first of all observes complexes. If he is content to continue to observe these complexes and to try to study their behavior in heredity, he may or may not obtain scientifically valuable results. But in any event, his safer course by far is to deal with part processes, first to analyze his complexes and then to select what seem to be the most important elements and carefully study their characteristics and their behavior as possible inheritances. From his own experience, the writer is inclined to urge that it is always safer to deal with items of behavior than to attempt to deal with behavior in a large or wholesale manner—safer, for example, to study capacity for a particular sort of musical expression, singing or violin playing, than to study musical ability in general.

Were we to present more examples from actual work, we might describe methods of studying distance orientation, visual discrimination, other aspects of habit-formation, the permanency of habits, instinct and emotion, in animals, but it will suffice for our present purposes to describe briefly two methods of analyzing behavior which have recently been devised. These methods, unlike those in general use by students of animal behavior, are applicable alike to man and to other mammals, even to birds as well. They were, indeed, planned with the idea that they should make possible the comparison of reaction-types or reactive

[6] Yerkes, Robert M., "Heredity of Savageness and Wildness in Rats," *Journal of Animal Behavior*, 1913, Vol. 3, p. 286.

tendencies in birds and mammals, and all these in turn with the tendencies displayed by human beings, either mature or immature, either normal or abnormal.

The two methods referred to are the quadruple choice method of Hamilton[7] and the multiple choice method of Yerkes. The Hamilton method places the subject in an experimental situation which may be reacted to in many different ways and with varying degrees of satisfactoriness or adequacy. The subject of the experiment is placed in a small room on one side of which there are four doors. From experience, he learns that he may escape by one of the doors, and only one, but which of the four to choose is his problem, for it is the plan of the experimenter to lock, in a given trial, the door through which the animal escaped in the previous trial and two others. Any one, then, of three doors may be unlocked in a given trial. The animal has absolutely no way of predicting which is unlocked. The general question is, then, how will a given type of organism or a given individual meet this situation? What habitual manner of meeting it will be acquired? How will the modes of reaction displayed by a child compare with those of an adult; of an ape, with those of a man?

The Yerkes method is similar in purpose to that of Hamilton, but it offers, in the opinion of the writer, somewhat more satisfactory opportunity to evaluate and compare results. It consists, essentially, in the presentation to the subject—bird or mammal; young or old, normal or abnormal—of a bank of twelve keys numbered from left to right, one to twelve. The subject is given to understand, verbally, or through actual experience with the apparatus, that pressing some one of the twelve keys will yield a certain desired result, such, for example, as the displaying of a picture, the presentation of food, the ringing of a bell. Success in the experiment means, simply, pressing the key which brings the desired result. The experimenter sees to it that in no two successive trials is the same key the one to be operated. He is, further, able to push back out of sight any number of keys and thus to present to the subject as few as one or as many as twelve.

Let us assume that in a given experiment the observer decides that the key the fourth from the left shall always be the "right" one. It then becomes the task of the subject of the experiment to suit his reactions to the number chosen by the experimenter. Only if he discovers the guiding idea of the experimenter can he succeed, trial after trial, in touching the right key at first. This method may be varied almost indefinitely in difficultness, and it may be made to elicit numerous reactive tendencies.

It is obvious that both of the methods thus briefly described above are attempts to elicit general reactive tendencies rather than to analyze reactions minutely and carefully. The methods are indeed intended to bring into clear light those modes of responding to a given situation which are characteristic of different types or conditions of living beings, and thus to furnish a basis for a profitable comparison of reactive tendencies.

I can not conclude this discussion without referring for a moment to a question which is frequently asked and which surely must have been in the minds of some of my hearers; namely, why is it that the behaviorist deals so often with the activities of the lower animals and so seldom with those of man? The question is pertinent, and the reasons, as I see them, are significant. They are chiefly two: in the first place, most lower animals are easily obtained,

[7] Hamilton, G. V., "A Study of Trial and Error Reactions in Mammals," *Journal of Animal Behavior*, 1911, Vol. 1, p. 33.

kept in confinement, bred and reared for experimental purposes; in the second place, many of them, in comparison with human beings, can be readily controlled throughout their lives and subjected to experimental conditions, in definite and measurable ways. Because, then, of the availability and controllability of lower animals, it is far easier and more satisfactory to make preliminary, exploratory and problems defining observations on their behavior rather than on that of man. It is further to be considered that the time of a human subject is worth infinitely more than that of an infra-human subject. On the whole, it seems clear that we work to advantage in the early stages of our science of behavior by letting the lower animals help us to the formulation of our problems and the development of our methods. Once fairly oriented and reasonably skilled in our technique, we may, with better effect, attack the problems of human behavior.

The above considerations lead to yet a further reflection concerning the relation of the study of the behavior of infra-human organisms to that of man. To the writer, it seems of preeminent importance that we prepare for rapid advance in our knowledge of human behavior by the systematic, thoroughgoing study of the behavior of some one or more of the anthropoid apes and of the higher monkeys. These creatures are nearest of kin to man, alike in structure and in behavior, and it is quite as surprising as it is unfortunate that we should know so little definitely concerning their mental characteristics or the facts and laws of their behavior. It may fairly be urged, I think, that no task comparable in importance with that of the systematic study of the instincts and intelligence of the apes lies before the behaviorist. Because of this strong conviction, I wish to present the following plan, which is quite as much in the interest of a study of man's behavior as of that of the anthropoid apes themselves.

It is proposed that a permanent station be established in some tropical country (Borneo and Jamaica would seem well worth considering) where, under favorable conditions, certain of the apes can be bred, reared and observed. Year after year, the staff of such a station should conduct systematic experiments with these animals and record observations of their behavior in their semi-wild state. There should be equally good opportunities for naturalistic and for experimental work, for the study of the development of forms of behavior, and of the relations of particular acts to definite environmental or other conditions. The value of such work would depend largely upon its continuance over a long period of time and upon the possibilities of breeding the animals and of observing the development of activities. To any one interested in the study of behavior, an elaborate program of research will at once present itself. It is wholly unnecessary, at this time, to enter into the details of such a plan. Suffice it to say that several biologists and psychologists, who have been consulted concerning it, enthusiastically approve of the proposal and earnestly hope that such a station may be established.

This plea for special and unique facilities for the systematic study of the apes is presented to you because upon students of genetics, eugenic investigators, and sociologists, quite as heavily as upon behaviorists and psychologists, must rest the responsibility of carrying out any such proposal. Moreover, I can urge the plan upon your consideration with enthusiasm because I fully believe that this apparently roundabout way to knowledge of the laws of our own behavior is in reality the most direct and desirable way. Certain it is that if

we neglect our present opportunities to study the anthropoids, our children's children will condemn us for neglecting invaluable opportunities. To-day, the chimpanzee, the orang-outang, the gibbon, as well as many species of monkey, are at hand for observation. A generation or two hence, many of the primates may be extinct. Should we not, in the interests of genetics, whether we be concerned primarily with problems of structure or of function, see to it that we adequately use, for the purpose of advancing human welfare, our present primate materials?

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SWEATING THE SCIENTIST[1]

IN the four last numbers of *Science Progress* a notice has been inserted asking for information on the emoluments of scientific workers; and a considerable number of interesting replies have been received. They are not numerous enough to form a basis for any statistical investigation of the subject—which it is hoped may be attempted later on when more evidence has been collected; but the replies received, combined with information which may be otherwise obtained, suffice to prove the low scale of payment given throughout the British Empire for such work.

The term "scientific worker" includes, according to the notice, all salaried workers—that is, men of all grades, namely, research students, assistants, professors, directors of laboratories, and other fully paid workers, and also half-time and whole-time workers. The duties generally include teaching and the administrative charge of university departments, museums and special laboratories. The lowest scale of pay mentioned in the replies is £85 a year for half-time work; but it is notorious that a large number of such workers, especially in medical subjects, are paid nothing at all. The pay of junior posts (which are also sometimes unpaid) rises from about £120 to £200, £250 and, rarely, £300 a year. These are of course not so important as the upper scales of pay for full-time professorships and permanent appointments. For the latter, the highest pay mentioned in the replies amounts to £850 a year, with a small pension (Ceylon). The next highest are salaries of £750, both in South Africa, and one of £500 in Canada, with small pensions generally contributed to by the holders of the appointments. It is well known that many professorships in Britain yield £600 a year, with very small contributory pensions. In no cases do there appear to be any arrangements for family pensions in the event of the holders' death—such as are often provided in the public services; nor insurance against illness or accident. Notoriously, very few even of the highest posts receive a salary touching or exceeding £1,000 a year; and in nearly all cases the pensions are contributory and are of a very small amount—retirement being often compulsory at the age of 60 or 65 years. Progressive rises of pay are also seldom provided for; so that a man who obtains an appointment when comparatively young can seldom hope for any increase during the rest of his life. Lastly, payment is laid down at many universities according to a flat rate, or according to fixed endowments which depend upon the funds originally allotted—so that no provision is made for retaining specially good men. In some cases holders of fully paid appointments are able to increase their emoluments by outside work. Many medical professorships are quite unpaid.

The rates of pay must be judged by the locality in which they are given. Thus £750 in South Africa is worth very much less than that sum in Britain, the cost of living being perhaps twice as great. A correspondent from Canada remarks that a salary of £800 a year in England is equivalent only to about £600 a year there, and is not sufficient for a professor. "A member of a learned community," he says, "can not live in a back street like a laborer, and if he takes an unfurnished house

[1] An editorial article printed in the April number of *Science Progress in the Twentieth Century: A Quarterly Journal of Scientific Work and Thought*, edited by Sir Ronald Ross.

in a good locality here the rent will be about a quarter of his income. . . . The smallness of income results, in my case, in my being unable to buy books, subscribe to scientific journals, or join all the learned societies I ought, or to travel to see other universities." Similar complaints are made from elsewhere; and the conditions in Britain are notorious.

Of course, very junior posts are generally financed by scholarships; and are naturally not highly paid because the holders are young men who are, practically, being apprenticed to their labors. The senior posts are those which must be considered in drawing any comparison between the payment for scientific work and other lines of effort; and even in this respect other conditions besides the payment must be taken into account. On the whole, however, such comparison leads to a very unfavorable conclusion regarding the present payment of scientific workers in Britain. It is bad, compared even with the Church. In middle posts, the salaries may be slightly higher; but in academical life the incumbents are obliged to live in towns and are rarely provided with housing. The highest appointments open in science certainly seem to be paid much less than the highest appointments in the Anglican Church—though the latter figures can not be very easily ascertained; and, at least, no scientific men have a seat in the House of Lords by virtue of their office or work. The highest salaries for scientific work are very much less than those given in the Army and Navy—which reach to £4,000 or £5,000 a year, and probably more when certain allowances are added. The scientific and academical sides of the medical profession show a similar state of affairs when compared with the clinical side—the incomes of the former seldom if ever exceeding £1,000 a year, while those of the latter are well known to run to many times that amount, especially in surgery. Compared with the law, science stands nowhere at all in Britain, either in payment or in position. The disparity is still greater in comparison with "business"; and the enormous fortunes made in innumerable directions by manufacturers, shipowners, retail and wholesale traders, vendors of registered articles, financiers, and so on, would in many single cases cover the whole funds allotted to science throughout the great British Empire. Even certain branches of art, such as the drama, singing and acting, have a large advantage compared with scientific work.

It is in no grudging spirit that men of science will draw such comparisons. That good pay should be given for good work is an elementary principle governing all lines of effort; but another principle must be held in view—that, if possible, payment should bear some proportion to the value of the kind of work done. We pay an architect or a general more than we pay the bricklayer or the soldier, because the labors of the former are the more important; and the same principle should carry weight in comparisons of the emoluments of the several professions. In the two previous numbers of *Science Progress*, a survey of the value of scientific work to the world has been attempted. It is probably of greater advantage to the world than any other line of effort. Science has become our premier industry, and governs every other industry just as the work of the architect governs that of the individual bricklayers. The world receives not only "fairy tales" from science, but also the most wonderful fairy gifts—a greater knowledge of the universe in which we live, a greater power over nature and over barbarism, greater precision in invention, in the treatment and prevention of disease, and in our manner of judging regarding all matters under discussion. Can it be truly said that the labors of any other professions are so valuable to mankind? Where the priest, the clinician, and the lawyer do good service to the few people surrounding them, and the soldier, sailor and politician do good service for their country, the discoverer confers benefits upon the whole world, and not for the present generation only, but for all times. We have already argued the case. Mathematics, chemistry, physics, physiology and pathology have practically built up all those great and wonderful additions which modern civilization has added to the civilization of the past, and,

with their sisters of the arts, have made a fitting palace for what ought to be a higher race. Yet the payment of the highly qualified men of the same age who were not so unwise and who are still perfecting them is less than that given to all the other professions, and, compared with the value of the work, is almost infinitely less. Indeed it would appear that the second principle enunciated above is just the opposite of the truth—that work is paid for in the inverse ratio of its value: and this is not a mere cynical gibe, but the actual truth. The greatest benefits which the world has ever received, that is, those which it has received from science, literature, art and invention, have generally been paid for not at all.

But it may now be said that the scale of payment for science is purely a question of supply and demand. That is so—and the same principle governs the case of sweated industries of all kinds. In the latter, the employer exploits the necessities of a crowded and poor population in order to have his work done at the cheapest rate. As regards science, however, the employer is the public itself, and the sweated laborer is the highest type of intellect in the country. The process by which the sweating is rendered possible is something as follows: Young graduates, fired with enthusiasm for science or with the desire of investigating some question which has occurred to them, take scholarships or poorly paid research-studentships. At first, while they are young, everything goes well with them; but after some years they find that the shoe begins to pinch. Then, unfortunately, it is too late. They have lost the time which they should have used in perfecting themselves for their proper profession, whatever that may be—in which they have already been outpaced by men who formed these sciences in the past or so high-minded as themselves. The opening which they may have taken five years previously is now closed to them; and they are compelled to spend the rest of their life under the paralyzing influences described above. This also is the actual fact; and it must evidently produce a disastrous influence, not only on the men who suffer, but also upon the great studies to which they devote themselves. The most capable graduates are already beginning to perceive the truth and to avoid the toils. The elder men, seeing that investigation leads to nothing, tend to interest themselves only in teaching, compilation of text-books, and attendance upon committees. The enthusiasm and concentration which when found together are called genius become impossible; and we look almost in vain for that high devotion to science which is the only quality she rewards with success. And the punishment does not really fall so heavily upon the worker himself—his enthusiasm for science may quite possibly compensate him for such troubles as those mentioned above. But the punishment falls upon his family; it falls upon the institution which employs him; it falls upon the nation which allows such a thing; and it falls upon science herself.

Besides the low rate of pay given, there are, in this country at least, many small abuses attached to high intellectual work. Even such funds as may be allotted are not used to the best advantage. Large portions of the income of many institutions are given to the maintenance of more or less useless pursuits—which were useful pursuits in the past, but no longer serve the world, or indeed serve it only in a negative sense. Originality and success in research do not receive their due place in selection for appointments. The best paid posts are seldom given for the best work done, but rather for qualities which are of little account—popularity, eloquence, text-book knowledge, private influence, and skill in the arts of time-service. For obvious reasons it is impossible to cite examples, but the fact remains. Of the few Britons of to-day who have done world-service, how many hold the leading public posts even in their own domain? We appear to judge men, not by the work which they have done, but by the work which we may imagine, from their appearance, that they may do if we give them an opportunity. How many of our most distinguished writers, for example, have received academic posts for teaching their own art? And how many of our most

distinguished men of science are now heads of British universities?

Many other disabilities are frequently complained of and resented by scientific workers. The whole system of filling appointments requires careful reconsideration. Some years ago an excellent article on the subject of advertising vacant appointments appeared in the *University Review*. The advertisements are often issued when the post has already been practically allotted—simply as a kind of show to prove impartiality on the part of the advertising body. The result is that numbers of candidates are tempted to put themselves to great trouble and some expense, and are kept upon the tenterhooks of doubt for months. Another abuse, still allowed for academical and hospital posts, is the necessity of canvassing for appointments—a very objectionable system which compels the unfortunate applicant to visit a number of persons with whom he is not acquainted and who often have no knowledge of his subject, and to parade his virtues before them in competition with other unfortunates who are in the same case. We heard some time ago of a distinguished mathematician who was obliged to sue humbly for a poorly-paid post before two local tradespeople—and who was not accepted. Can anything show more clearly than such a state of affairs the low position held by high work in Britain? Indeed the whole system so frequently adopted here of allowing scientific institutions, hospitals and even universities, to be governed by committees of persons of whom many have no qualifications for the work, who are often not even moderately distinguished in any line, but who find their profit in the position, is thoroughly discreditable; and recent disputes in the management of certain hospitals have illustrated the defect.

We have recently started the habit of giving our rare professorships to foreigners—not really because the foreigners are the best men for the posts, but because the institution concerned likes to obtain a reputation for magnanimity. Yet foreign nations are not so generous to us. As a matter of fact we buy, not in the cheapest market, but in the dearest one; and do so, not from motives of business, but merely out of ostentation. The same indifference to work done is often manifested in the honors given by many learned bodies. We see the academic laurel placed upon the brows of soldiers, sailors and politicians—men who have perhaps done great service in their own line, though not in the line for which such honors should be reserved. The case can of course be argued—as all bad cases can; but it is really a matter of clean taste. Academic honors are meant to promote great world-service; and it is a sign of national degeneracy when they are given for anything lower. One would think that our universities would lead the way in this respect, but it is not so. Some years ago a distinguished colonial premier refused an academic honor on these grounds, and attained great honor by doing so. Few are the struggling workers or the struggling causes which have benefited by the powers in the hands of the great learned bodies. To add grist to their own mill by subserviency to popular idols appears too often to be their chief desire; and where a great worker is honored by them, he is generally a foreigner. A still lower stage, however, has already been reached—where a learned body decorates itself!

We may now ask, what exactly does the British Empire do, as a state, for science, or indeed for any of the higher forms of intellectual effort? Parliament allots £4,000 a year to one learned society, and another £1,000 a year for publications—a magnificent endowment! It allows also occasional small grants to other institutions; and all these are doled out for the expenses of special researches. The larger grants which it gives to universities are devoted chiefly to teaching—a very small proportion ever being really available for investigation. Very little of the money goes to the workers themselves, either to increase their pay or to reward them for services rendered; and the state seems to think that if it provides their test tubes and microscopes it has done enough. In many countries the government wisely pays members of certain academies; but in Britain, not only is this not done, but the state actually exacts gratuitous

services from such members. For example, a government department wishes for expert advice on some matter—it ought to form a commission of its own and honestly pay the expert members of it. Instead of doing this the government department goes to some learned society and asks it to advise on the scientific question at issue. The society is honored by the request, and obtains the advice gratis from its own members. Thus the government gets what it requires for nothing; the learned body is overpowered with the honor rendered to it; and the unfortunate worker is the loser. Such action is very common; unpaid government committees are now becoming the rule, and even reimbursement of traveling expenses is often boggled at. We heard the other day of a man who was actually found fault with for not attending a committee of this nature for which he was not paid. In other words, the state exploits the man of science on account of his enthusiasm for his work and his patriotism. The thing might be excused if the state were to give large funds for scientific work, but as it does not do so such action is extraordinary in its meanness and impropriety.

Many similar points may be cited. The board of education expends annually an enormous sum, amounting to nearly twenty millions a year, on low-class education; but what does it do for the greatest of educators—science, literature, art, drama, exploration, discovery, invention? As was pointed out in the last issue of *Science Progress,* the patent acts do not cover those whom they should most carefully protect, namely the men upon whose investigations nearly all inventions are founded. Quite recently the House of Commons has given itself payment amounting to over a quarter of a million pounds a year. Perhaps this is quite right; but may we not ask whether a small fraction of the money, properly devoted to scientific investigation in many lines, would not be of much greater benefit to the people than are the wranglings of party politicians over questions which will never be honestly decided because they are never honestly considered? Still more recently the state has given, very wisely, £57,000 a year out of the insurance fund for medical researches. It was suggested at the committee which organized the management of this expenditure that a large prize should be available out of the fund for important discoveries; but the money actually offered has now been reduced to a maximum of £1,000. In other words, if a private medical man were to discover the means of prevention or cure of tuberculosis or cancer—which he would not be likely to do without spending years of study over the theme, and probably losing his practise in consequence of his work—his only reward would be £1,000! The discoverer will not be paid; and yet the country hopes to have discoveries achieved! And this brings us to what is really the crowning defect of the national attitude towards high effort of such kinds, namely, that it makes no attempt whatever to pay for any benefits, however great, which it receives from individuals. A successful soldier may indeed receive a handsome donation, and many politicians obtain large pensions; but the highest services in the domains of science, literature and art are not deserving of reward!

The net result may of course be foretold from these data. There is much petty science, petty literature and petty art; but the more arduous labors which require the devotion of a lifetime are becoming increasingly difficult. The man of science is now exactly in the position in which writers and inventors found themselves before the copyright and patent acts were passed. He is never the master in his own house; he is the slave to institutions which "run him" for what he is worth; and is seldom able to spend his time in the exercise of the lofty gift which nature has given him. Still worse, the most capable minds are at the outset turned away from fields in which their efforts are likely to be of the highest value to humanity.

All this really springs from the curious and stupid attitude of the public towards all forms of intellectual effort. It seems to take no interest in such effort. Politics, game-playing and picture-shows are the things which amuse it. The great worker is a mere book-

worm, or a plodder, or a crank. But the truth is that, just as individuals have duties to perform to their country, so have countries duties to perform to the civilized world. It is the duty of every nation to participate in the discovery of the laws of nature, to ascertain the cause of disease, to enhance the powers of man, and to widen the range of his vision. What does Britain do to fulfil this duty? She still has great workers, it is true; but their work springs from themselves, and not from the nation. The country does not perform the duty referred to. It has become like a tradesman who has reached great wealth by the exercise of inferior arts, but who spends it on amusements, pleasures and the ostentation of charity, without sparing a penny for higher objects. This figure may at least be reached as a rough integration of the general complex formulæ of our present condition. Behind all there is a shadow: for nations, like individuals, must remain efficient.

EFFECT ON THE PROPAGATION OF ELECTRIC WAVES OF THE TOTAL ECLIPSE OF THE SUN, AUGUST 21, 1914

THE committee for radiotelegraphic investigation of the British Association for the Advancement of Science calls attention to the fact that the forthcoming total eclipse of the sun affords an exceptional and important opportunity of adding to existing knowledge of the propagation of electric waves through air in sunlight and in darkness, and across the boundaries of illuminated and unilluminated regions. The eclipse will be total along a strip extending from Greenland across Norway, Sweden, Russia and Persia to the mouths of the Indus. In Russia the duration of totality will be a little more than two minutes.

There are two main points calling for investigation during the eclipse. In the first place, the propagation of signal-bearing waves through air in the umbra and penumbra will probably obey laws different as regards absorption and refraction from those obeyed in illuminated air. In the second place, the strength, frequency and character of natural electric waves, and of atmospheric discharges, may vary. The variations may occur either because the propagation of natural waves from distant sources is facilitated or impeded by the eclipse, or, possibly, because the production of natural electric waves or atmospheric discharges is for some unknown reason affected by the eclipse.

These points have previously been investigated to only a slight extent. The observers of signals during the solar eclipse of April 17, 1912, nearly all agreed that the strength of the signals was greater during the eclipse than an hour before or after. There was only one special observation of strays during the same eclipse, when very pronounced and remarkable variations were recorded during the passage of the shadow-cone across Europe.

To investigate the propagation of signals across the umbra it will be necessary to arrange for wireless telegraph stations on either side of the central line of the eclipse to transmit signals at intervals while the umbra passes between them. This transit of the umbra occupies about two minutes. It is thus very desirable that the Scandinavian and Russian stations should transmit frequently throughout several minutes before, during and after totality. But stations other than those favored by their proximity to the central line should endeavor to keep a complete record of the variations of signals during the eclipse. Stations in Europe west of the central line and stations in the Mediterranean and in Asia Minor may find noticeable changes in the strength of signals, particularly long distance signals, between the hours of 10 A.M. and 3 P.M., Greenwich time; and it is probable that the stations of India and East Africa, and ships in the Indian Ocean, may feel the effect of the penumbra in the afternoon. On the other hand, ships in the Atlantic, and fixed stations in Eastern Canada and the United States, will probably be affected by the penumbra in the early morning. At Montreal the eclipse (partial) is at its greatest phase at 5:52 A.M. standard time. It is possible that the eclipse may have some influence even when it is invisible.

The investigation of strays is of as great

interest as that of signals. So far as is yet known, the natural electric waves reaching wireless telegraph stations in latitudes higher than 50° appear to travel mostly from the south. Thus the greatest changes produced in strays by the eclipse will probably be experienced at stations in Scandinavia and Russia, to reach which the waves must cross the path of the umbra. At the same time changes of some kind are to be expected in other districts than these, and it is therefore desirable that statistical observations of natural electric waves be made all over the world, and especially at places within an earth quadrant of southern Russia. It is also desirable that meteorological observations, including those of atmospheric ionization and potential gradient, should be at the disposal of the committee when considering the records of strays and signals.

The committee proposes to prepare and circulate special forms for the collection of statistics of signals and strays, especially within the hemisphere likely to be affected by the eclipse; they will endeavor to make provision for the transmission of special signals at times to be indicated on the forms; and they will offer for the consideration of the authorities controlling stations near the central line a simple program of work. The discussion of the observations, and the comparison with meteorological data, will be carried out by the committee; and digests of the statistics, together with the conclusions drawn from the analysis, will be published in due course.

The committee would be greatly aided in the organization of this investigation if those possessing the necessary facilities and willing to make observations during the eclipse would communicate with the honorable secretary, Dr. W. Eccles, University College, London, W. C., at the earliest possible date.

THE NAPIER TERCENTENARY CELEBRATION

John Napier's "Logarithmorum Canonis Mirifici Descriptio" was published in 1614; and it is proposed to celebrate the tercentenary of this great event in the history of mathematics by a congress, to be held in Edinburgh on Friday, July 24, 1914, and following days.

The celebration is being held under the auspices of the Royal Society of Edinburgh, on whose invitation a general committee has been formed, representing the Royal Society of London, the Royal Astronomical Society, the town council of Edinburgh, the faculty of actuaries, the Royal Philosophical Society of Glasgow, the universities of St. Andrews, Glasgow, Aberdeen and Edinburgh, the University College of Dundee, and many other bodies and institutions of educational importance.

Through the favor of the editor of Science, the president and council of the Royal Society of Edinburgh have now the honor of giving a general invitation to mathematicians and others interested in this coming celebration.

The celebration will be opened on the Friday with an inaugural address by Lord of Appeal Sir J. Fletcher Moulton, F.R.S., LL.D. (Edin.), followed by a reception given by the Right Honorable the lord provost, magistrates and council of the city of Edinburgh. On the Saturday and Monday the historical and present practise of computation and other developments closely connected with Napier's discoveries and inventions will be discussed. A memorial service will be held in St. Giles' Cathedral on the Sunday.

Among many who have expressed a warm interest in the celebration and who hope to take part in the congress, may be mentioned Professor Andoyer, Paris; Professor J. Bauschinger, Strassburg; Professor Hume Brown, Historiographer Royal for Scotland; Professor F. Cajori, Colorado, U. S. A.; Professor G. A. Gibson, Glasgow; Dr. J. W. L. Glaisher, Cambridge; Professor Lang, St. Andrews; Professor Macdonald, Aberdeen; Professor E. Pascal, Naples; Professor Karl Pearson, London; Professor Eugene Smith, New York; Professor Steggall, Dundee; Professor Whittaker, Edinburgh.

Merchiston Castle, the residence of Napier, has long been occupied by the well-known public school, which draws pupils from all parts of the British empire. The governors of

the school have kindly invited the members of the congress to visit the castle and grounds on the Saturday afternoon.

Relics of Napier, collected by Lord Napier and Ettrick and other representatives of the family, will also be on view; and it is intended to bring together for exhibition books of tables and forms of calculating machines, which may reasonably be regarded as natural developments of the great advance made by Napier.

Individuals, societies, universities, public libraries, etc., may become founder members on payment of a minimum subscription of £2; and each founder member will receive a copy of the memorial volume, which will contain addresses and papers read before the congress, and other material of historic and scientific value. It is important to secure as many founder members as possible, so that a volume may be brought out worthy of the memory of Napier.

Ordinary subscribers attending the celebration may receive copies of the memorial volume at a reduced price.

Subscriptions and donations should be sent to the honorary treasurer, Mr. Adam Tait, Royal Bank of Scotland, St. Andrew Square, Edinburgh.

All who are interested in this proposed celebration are respectfully invited to communicate with the general secretary of the Royal Society of Edinburgh, 22 George Street, Edinburgh, and to announce their intention of being present.

C. G. Knott,
General Secretary

Royal Society of Edinburgh

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The council met at the Cosmos Club, Washington, on Tuesday, April 21, 1914, at 5:45 P.M. In the absence of the chairman, Mr. Diller presided. Those present were: Messrs. Bowie, Cattell, Diller, Gulliver, J. A. Holmes, Howard, Humphreys, D. S. Johnson, John Johnston, Kober, Metcalf, Pickering, Shear, Trelease and Twitmeyer.

The financial report of the permanent secretary was read and, on motion, was approved and ordered printed.

Mr. Cattell submitted the report of the committee on policy.

On motion, the following resolutions recommended by the committee on policy were adopted *ad seriatim:*

1. *Resolved,* that Professor Pickering be added to the committee on policy.

2. *Resolved,* that Dr. E. W. Allen, of the office of experiment stations, U. S. Department of Agriculture, be elected secretary of Section M (Agriculture) and that he be authorized, with Professor L. H. Bailey, vice-president of the section, to nominate the sectional committee for Section M and also its representative on the council.

3. *Resolved,* that the permanent secretary be instructed to inform Dr. Robert M. Ogden that no funds can be provided for the present for the associate secretary for the south beyond necessary clerical expenses.

4. *Resolved,* that the first of the Jane M. Smith life memberships be awarded to Professor Charles Henry Peck, of Albany.

5. *Resolved,* that the entrance fee be remitted to members of the Pacific Association of Scientific Societies who join the American Association during the present year.

6. *Resolved,* that the council authorize a grant of four hundred dollars to the Pacific coast committee for the expenses of its membership committee.

7. *Resolved,* that the council authorize the Pacific coast committee to adopt the constitution for the Pacific Division and that it be suggested that the second clause of the second paragraph of Article 3, beginning with the word "except," of the constitution submitted, be omitted.

8. *Resolved,* that the council give the treasurer full power to act for the association in the Colburn will case.

9. *Resolved,* that the council authorizes the appointment of Messrs. Eliot, Minot and Pickering as a committee to take the necessary steps to amend the charter of the association increasing the amount of property which may be held by the association.

10. *Resolved,* that the clerical expenses of the committee of one hundred on scientific research and its subcommittees be met from the income from the permanent funds of the association.

11. *Resolved,* that the council welcomes the action of the council of the American Chemical So-

ciety favoring the plan of holding meetings jointly with the American Association at four-year intervals.

12. *Resolved*, that the publication of the constitution and list of members be postponed until January, 1915.

On motion, it was resolved that the sectional committee of Section C be empowered to elect its vice-president and chairman for the Philadelphia meeting in consultation with the committee on policy.

L. O. HOWARD,
Permanent Secretary

SCIENTIFIC NOTES AND NEWS

DR. GEORGE WILLIAM HILL, distinguished for his contributions to mathematical astronomy, has died at the age of seventy-six years.

DR. CHARLES SANTIAGO SANDERS PEIRCE, known for his work in logic and mathematics, died on April 19, aged seventy-four years.

THE National Academy of Sciences on April 22 presented its "medals for eminence in the application of science to the public welfare," to Colonel George Washington Goethals and Brigadier General William Crawford Gorgas. The presentation was made by Dr. William H. Welch, president of the academy, at a dinner held in honor of the retiring president, Dr. Ira Remsen, and the retiring home secretary, Dr. Arnold Hague.

MEMBERS of the National Academy of Sciences were elected at the annual meeting on April 23 as follows: Ernest Merritt, physicist, Cornell University; Moses Gomberg, chemist, University of Michigan; Edward Curtis Franklin, chemist, Stanford University; Frederick Leslie Ransome, geologist, U. S. Geological Survey; Nathaniel Lord Britton, botanist, New York Botanical Garden; Henry Herbert Donaldson, neurologist, Wistar Institute of Anatomy; Herbert Spencer Jennings, zoologist, The Johns Hopkins University; Francis Gano Benedict, chemist, nutrition laboratory of the Carnegie Institution; Walter Bradford Cannon, physiologist, Harvard University; Jesse Walter Fewkes, ethnologist, Bureau of American Ethnology.

AT its annual meeting in Philadelphia on April 25, the American Philosophical Society elected to membership the following residents of the United States: Charles Greeley Abbot, Washington; James Wilson Bright, Baltimore; Bradley Moore Davis, Philadelphia; Thomas McCrae, Philadelphia; William Diller Matthew, New York; Alfred Goldsborough Mayer, Washington; Samuel Jones Meltzer, New York; John Campbell Merriam, Berkeley; Robert Andrews Millikan, Chicago; William Albert Noyes, Urbana; Stewart Paton, Princeton; Richard Mills Pearce, Philadelphia; Palmer Chamberlaine Ricketts, Troy; Harold A. Wilson, Houston; Frederick Eugene Wright, Washington. Foreign residents were elected as follows: Shibasaburo Kitasato, Tokyo; Heike Kamerlingh Onnes, Leyden; Vito Volterra, Rome.

PROFESSOR MOSES GOMBERG, of the University of Michigan, has been given the gold medal of the New York section of the American Chemical Society for his work on the trivalence of carbon.

PRINCE GALITZIN will preside over the fifth meeting of the International Seismological Association, to be held in St. Petersburg in September.

DR. J. D. FALCONER, lecturer in geography in the University of Glasgow, has been appointed by the trustees of the British Museum to the Swiney lectureship in geology, in succession to Dr. T. J. Jehu.

OLIVER BOWLES, of the department of geology and mineralogy, University of Minnesota, has been appointed quarry technologist in the division of mineral technology, Bureau of Mines, Washington, D. C.

DURING the absence abroad of Mr. William H. Fox, director of the museums of the Brooklyn Institute of Arts and Sciences, Mr. Robert Cushman Murphy, curator of mammals, is acting director.

THE German geographer, Dr. Ewald Banse, has started on an expedition for the exploration of the Libyan desert.

B. M. PATTEN, Ph.D., has received an appointment from the United States Fisheries

Department to go on the *Seneca*, a government patrol vessel, and make a study of the temperature, salinity and other qualities of the water of the ocean at various depths.

Drs. S. Marks White and Jennings C. Litzenberg, of the medical faculty at the University of Minnesota, are absent on leave the current semester. Dr. Frederick H. Scott, of the department of physiology, has been granted leave for the first semester of 1914–1915, and Professor Harold E. Robertson, of the department of pathology, has been granted a year's leave of absence during 1914–1915.

Professor B. Shimek, of the department of botany of the State University of Iowa, will carry on research work in Europe during the remainder of the year. He has been invited to deliver a series of twelve lectures, chiefly on the subjects of the plant ecology of the American desert and prairie, and the loess, at the University of Prague during the summer semester, 1914. He will also present two papers, by request, before the meeting of biologists and physicians to be held at Prague from May 31 to June 3.

Professor A. N. Winchell, of the University of Wisconsin, has returned from a visit to the Missouri School of Mines, the University of Kansas, the Agricultural College of Iowa and the University of Illinois. At each institution he delivered two lectures, one upon the "Mining Geology of the Butte District," and the other upon the "Origin of the Butte Ore Deposit."

The Syracuse Chapter of Sigma Xi has held two open meetings during the past winter. At the first, on February 20, Professor M. I. Pupin, of Columbia University, gave an address upon the topic, "Wave Conductors," with especial reference to the so-called Pupin conductors which have made present-day long-distance telephony possible. At the second open meeting, March 6, Dr. Robert Almer Harper, professor of botany at Columbia University, gave an address upon the topic, "Studies in Morphogenesis." A large audience consisting of both university and city people, greeted each lecturer.

Professor E. Heyn, of Berlin, is this year to deliver the annual May lectures before the Institute of Metals, London, upon the subject, "Internal Strains in Cold Wrought Metals."

The Friday evening meetings of the Royal Institution were resumed on April 24 when Dr. F. W. Dyson, the astronomer royal, lectured on the stars around the north pole. On May 8, Professor Karl Pearson will give an address on albinism in men and dogs; and on May 15 Professor Keeble will speak on "Plant Animals: A Study in Symbiosis."

The department of anatomy at the University of Minnesota has issued two volumes of reprints of anatomical literature, by the following members of the department: J. B. Johnston, R. E. Scammon, W. F. Allen, W. A. Hilton, E. T. Bell, T. G. Lee, W. S. Nickerson, Robert Retzgar. Volume I. covers 1909–11 and Volume II., 1912–13.

The University of Michigan has begun the publication of scientific papers of the museum of zoology, under the general title "Occasional Papers of the Museum of Zoology, University of Michigan." The papers are to be published separately, at irregular intervals, and will be numbered consecutively. The whole series will be sent through the university library to libraries and scientific institutions of a zoological nature, and the museum will distribute copies of each number to students interested in the subjects discussed.

The twenty-third session of the Marine Biological Laboratory, of Stanford University, will begin on Monday, May 25. The regular course of instruction will continue six weeks, closing July 4. Investigators and students working without instruction may make arrangements to continue their work through the summer. The laboratory will be under the general supervision of Professor G. C. Price, instructor in charge. The laboratory provides for three classes of students: (1) Teachers and students who have not had the advantages of laboratory courses in zoology. (2) Advanced students in zoology and physiology who wish to continue their studies. (3)

investigators who are prepared to carry on researches in morphology or physiology.

A SERIES of water-color plant studies painted by the late traveler and artist, Miss Adelia Gates, and presented to the Smithsonian Institution by her niece, Miss Eleanor Lewis, of Yellow Springs, Ohio, is now exhibited in the new building of the U. S. National Museum. The subjects treated embrace a wide range of foreign and domestic plants painted by the artist in this country and abroad. Some additions have recently been made to the original deposit by the donor who has endeavored to bring together all the flower studies made by Miss Gates, many of which were widely distributed by her prior to her death. With these additions, the collection now numbers some 600 paintings.

WE learn from *Nature* that a collection of rock specimens of considerable historic interest has been presented to the department of minerals of the British Natural History Museum. The specimens in question were collected in Arctic North America by Sir John Richardson, who accompanied Sir John Franklin's Arctic expeditions of 1819–1827. They have since that time been kept in the museum of the Royal Naval Hospital at Haslar, but inasmuch as the fossils collected in the same Arctic expeditions are in the National Museum at South Kensington, it was felt to be in the fitness of things that the rocks should be also preserved there. An application was accordingly made to the Lords of the Admiralty to sanction the transfer of the specimens.

FROM June 23 to 30 there will be held in London, as we learn from the London *Times*, the third annual International Congress on Tropical Agriculture, promoted by l'Association Scientifique Internationale d'Agronomie Coloniale et Tropicale. This society was formed with the idea of helping associations formed in tropical colonies for the development of their agriculture to study in common fundamental problems connected with the successful growing of important natural tropical products, such as rubber, tea, coffee, cocoa, tobacco, cocoanut oil, cotton, jute, sisal hemp and cinchona. The value of the results expected from the congress may be gauged by the fact that on the organizing committee practically every tropical colony in the British Empire is represented by its principal agricultural officer. Official notifications are being sent out by the British Foreign Office and the Ministère des Affaires Etrangères in Paris to the governments of all countries possessing tropical colonies inviting them to appoint official delegates. The congress is to meet at the Imperial Institute, South Kensington, and among the subjects suggested for papers and discussion are: Technical education and research, labor organization and supply, rubber production, development of cotton-growing, fiber production, agricultural credit banks, agriculture in arid regions, tropical hygiene and preventive medicine, plant diseases and pests, and so forth. Numerous papers on these and kindred subjects have been promised by well-known experts. Professor Wynham R. Dunstan is the chairman of committee, and Dr. T. A. Henry and Mr. H. Brown, of the Imperial Institute, are the honorary secretaries.

Two models which show how the government sells its timber have just been prepared in Washington, for display at the forest products exposition, to be held in Chicago, April 30 to May 9, and in New York, May 20 to 30. These models represent an acre of western yellow pine land in a national forest of the southwest before and after logging. In the model showing the stand before the lumberman goes into it the trees range from those only a few years old to large, overmature, stag-headed individuals more than ready for the axe. In the second model the mature trees and all others larger than a certain diameter have been cut down and made into logs and cord-wood. In this, as in all government sales, the stumps are cut low to avoid unnecessary waste, logs are taken to a small diameter well up into the tree, and such material as is not fit for lumber is converted into cordwood. Together, the models show the care which the government requires of lumbermen in felling

old trees so that the young growth is not injured. The brush is piled in heaps for burning after the lumber has been removed, in order that the fire menace which foresters say usually follows lumbering may be done away with. The models are on a scale of about one inch to five feet, so that trees approximately 100 feet tall are about 20 inches high in the models. It is the intention of the forest service to have these models set forth certain points in regard to the timber sales conducted by the government. They will demonstrate, in the first place, that the timber on the national forests is for use; placards tell how it is sold to the highest bidder when it is wanted for commercial purposes, and how it may be given away to local settlers and prospectors for developing homesteads or mines. The models themselves indicate that the timber is cut in such a way as to eliminate all avoidable waste of wood and to secure a continuance of the forest crop, not only for timber production but for the protection of the soil. They further show the fundamental principles applicable to many logging operations, that the mature and overmature trees should be removed, that thrifty growing young trees should be left to produce seed and insure a reproduction of the stand, and that the young growth should be protected from damage in lumbering operations. The models are supplemented by a graphic chart, which shows by pictured piles of money and by conventional trees of graded size the increase of timber sales on the national forests from 1905 to 1913, inclusive. In 1905 the timber sold from the national forests aggregated 96,000,000 board feet, which brought the government no more than $85,000. Three years later the amount of timber sold increased to nearly 390,000,000 board feet, and the money received rose to $735,000. In 1911 830,000,000 board feet sold for more than $2,000,000, and in 1913 more than 2,000,000,000 feet brought in contracts amounting to $4,500,000. Not all this money was received in any one year, because national forest timber is sold on contracts which range from one to twenty-five years, and it is paid for as cut.

UNIVERSITY AND EDUCATIONAL NEWS

The Catholic University of America, Washington, will receive the greater part of the estate of $1,000,000 left by Theodore B. Basselin, of Croghan.

Mr. James Deering, in a letter addressed to the trustees of Northwestern University and of Wesley Hospital, announces a gift of $1,000,000 to the hospital. It is provided that Wesley Hospital shall be a teaching hospital under Northwestern University. The gift is made in honor of the donor's father, the late William Deering, and his sister, Abbie Deering Howe, who died in 1906.

An anonymous benefactor has given a sum of $1,200,000 to the municipality of Berlin for the foundation of an open-air school for boys. In the course of instruction special attention will be given to modern languages and natural science.

Arthur Taber Jones, Ph.D., has been appointed assistant professor of physics at Smith College.

Henry Laurens, Ph.D., has been appointed instructor in biology, and George A. Stetson, instructor in mechanical engineering at Yale University.

At Harvard University, Dr. W. E. Hocking, professor of philosophy at Yale University, has been appointed professor of philosophy, and Dr. R. F. A. Hoernle, of the University of Durham, assistant professor of philosophy. Dr. E. M. East has been promoted to be professor of experimental plant morphology and H. J. Hughes, to be professor of civil engineering. Dr. S. B. Wolbach has been promoted to be associate professor of bacteriology and Dr. C. L. Bouton to be associate professor of mathematics.

The executive committee of the Massachusetts Institute of Technology has made the following promotions in the instructing staff: From the grade of associate professor to the grade of professor, Warren K. Lewis, in chemical engineering; George B. Haven, in machine design; Samuel C. Prescott, in industrial biology, and Charles B. Breed, in railroad engineering. The following assistant

professors have been advanced to the grade of associate professor: Carroll W. Doten, in economics; A. A. Blanchard, in inorganic chemistry; S. M. Gunn, in sanitary biology and public health; A. T. Robinson, in English; A. G. Woodman, in food analysis. The following instructors have been advanced to the grade of assistant professors in the departments indicated: Charles W. Green, in electrical engineering; Henry H. W. Keith, in naval architecture; John F. Norton, in chemistry of sanitation; Joseph W. Phelan, in inorganic chemistry; George W. Swett, in machine design, Frederick H. Lahee in geology. Assistants advanced to the grade of instructors are: Ralph G. Adams, in mechanical engineering; Arthur E. Bellis and Charles L. Burdick in theoretical chemistry; Edward A. Ingham in biology; Norman Osann in electrical engineering, and DeWitt M. Taylor, in mechanical engineering.

DISCUSSION AND CORRESPONDENCE

INTERPRETATIONS OF THE ANOMALIES OF GRAVITY

UNDER this title Mr. G. K. Gilbert discusses[1] the investigations of Messrs. Hayford and Bowie[2] (of the Coast and Geodetic Survey) relating to terrestrial gravity, and its application to observed earth movements by J. W. Spencer.[3] Any consideration of such important new problems should be welcomed as they tend to confirm previous results, or show their weakness or the lack of information.

Hayford and Bowie have stated that, for the purpose of making computations, the earth's crust is assumed to be in a state of perfect isostacy. They show, contrary to Gilbert's ideas on the subject, that while this is true for the whole area of the United States, there are large areas where the anomalies depart slightly from the perfect balance and smaller areas where the anomalies are considerable. This last is the special feature of Spencer's application of the anomalies of gravity to earth movements. Thus, at Washington, the excess of gravity is equal to 1,200 feet, while the deficiency at Virginia Beach (160 miles distant) is equivalent to a thickness of 1,600 feet of rock.

Hayford and Bowie have found that the topography is all compensated within a depth of 122.2 kilometers below sea-level (although they used 113.7 km. in their gravity computations); that is, the condition of stress at and below the depth of compensation is isostatic, or in other words "any element of the mass is subject to equal pressures from all directions as if it were a perfect fluid." Gilbert has misconstrued their conception of this, for he states "immobility at all depths below that of compensation is either explicitly or implicitly assumed by Hayford and Bowie." He also appears to take the view that even very small areas are completely compensated, and that much of the compensation in the vicinity of the stations, with decided or large anomalies, is located in the nucleus. This view is untenable as shown by such anomalies as those of Washington and Virginia Beach, or still greater ones between Olympia and Seattle. This compensation, located in the nucleus, presupposes very high rigidity, which is contrary to the idea of complete local isostacy, which on the other hand presupposes great plasticity.

Gilbert discusses the causes of the anomalies, favoring the one based upon the local variation of density of the column and heterogeneity of the nucleus with a sub-crustal mobile layer, which accounts for the isostacy. But all materials of the earth's crust are mobile under long-continued stress differences, yet there is sufficient rigidity in the crust to sustain local anomalies.

In his discussion, Gilbert assumes that the nucleus is composed of the same materials as

[1] U. S. Geological Survey, Professional Paper 85-*C*, pp. 29–37.

[2] Hayford, J. F., and Bowie, William, "The Effect of Topography and Isostatic Compensation upon the Intensity of Gravity," U. S. Coast and Geodetic Survey Special Pub. No. 10, Washington, 1912; Bowie, William, "Effect of Topography and Isostatic Compensation upon the Intensity of Gravity," *id.*, Special Pub. No. 12, Washington, 1912.

[3] Spencer, J. W., "Relationship between Terrestrial Gravity and Observed Earth Movements of Eastern America," *Am. Jour. Sci.*, 4th ser., Vol. 35, pp. 561–573, 1913.

those of the crust, and that the great density of the earth (5.6) is due to "compression by pressure," in spite of the remarkable incompressibility of even water, with the interior heat acting contrariwise. The most commonly accepted view of the great weight of the nucleus of the earth is that it is composed of heavy metallic substances; for instance, astronomer Ball regards meteorites as the remains of disrupted planets such as would be liberated by the explosion of the earth.

Concerning the relationship between the anomalies of gravity and earth movements, Dr. Gilbert says:

> Spencer emphasizes the fact that there are large plus anomalies within the region once covered by the Laurentian ice and regards it as proof that the rising of the region after the removal of the ice load was not caused by the removal of the load.

Again he says:

> The fact (of plus anomalies within this area) may equally be used to discredit the hypothesis underlying his mode of interpreting anomalies.

These statements give neither the facts nor arguments upon which Spencer bases his hypothesis that the anomalies are not due to the removal of an ice load, nor how the facts discredit his hypothesis. Observing that the plus anomaly (equaling 700 feet of rock) north of the Adirondacks, and the deformation (of 650 feet) of the earth's crust as seen in the tilted beaches, closely agree, Spencer naturally concluded that there is a direct relationship between the two phenomena. Farther south in the Adirondacks, composed of dense rocks, the anomaly of gravity is reduced to 200 feet of rock. Southward from this and extending over a very great region once covered by ice the anomalies show deficiency of weight. If the deformation adjacent to the St. Lawrence River were due to the removal of the ice sheet, then the region to the south should also have been elevated to isostate equilibrium.

Supporting Spencer's conclusions, from evidence lying outside of the glaciated region, the Appalachian belt and Florida are overweighted, although much material has been removed from the mountains. On the other hand, the coastal region is found to be underloaded, although it is here that the deposition of the materials, brought down from the mountains, have accumulated. This underloading agrees with the subsidence shown by the canyons and valleys indenting the submarine border of the continent. Yet this collateral evidence is not considered by Gilbert.

The observation of all these features is of comparatively recent date, yet they have the greatest value, although they are contrary to the hypothesis that the mobility of the earth's crust is so complete that areas of considerable size can not either be loaded or unloaded, without being fully accounted for in the isostatic balance. The phenomena of earth movements and of anomalies of gravity introduces new features in the evolution of our continents, which have only begun to be investigated.

It may be added that Professor Leverett and also Mr. Taylor have just announced that they have found moraines in the lake region, in disagreement with the hypothesis that the deformation of the earth's crust is due to the removal of the ice—results in accord with relationship of the anomalies of gravity and earth movements as lately first described by the present writer.

J. W. Spencer

HEADS OF DEPARTMENTS: A COMMENTARY UPON DR. JOHNSTON'S ARTICLE

It was very wholesome reading that Doctor Johnston offered the heads of departments in his article upon University Organization, appearing in the December 26 issue of Science, p. 908. The unfortunate conditions described so truly he evidently finds existing not in any one special institution, but in many.

Any fair-minded head realizes the disadvantages under which younger members of his staff labor. Unfortunately, there are many professors who are quite content to allow their associates to remain unheard and unheeded, either because they honestly (and ignorantly) believe them lacking in wisdom or because they fear the effect of allowing them to be in the least prominent. For such, as well as for the more liberally inclined,

Doctor Johnston's frank utterances are of benefit and form an integral part of many present-day expressions which, no doubt, will result in bettering conditions in universities.

Some readers of the article referred to might perhaps have desired more detailed expression in the author's constructive paragraphs. He fails, in the estimation of the undersigned, to give in sufficient detail, suggestions for the relief of the situation in general existing between a head of department and his staff, although it is this feature which he particularly criticizes in the article referred to. In just what position, for example, should a head regard his men, with reference to their responsibilities? In how far would the Doctor make them independent of the head, that is, free to act upon their own intiative, without first obtaining the sanction of the chief, etc.?

A detail included in Doctor Johnston's broad generalizations and annually clamoring for relief is as follows: A head, in recommending a member of his staff for advancement, either in rank or salary, is almost invariably and perhaps sometimes unconsciously influenced by prejudice. The man of pleasing personality or with a possible close social connection or representing a particular phase of the work in which the head is interested is the one recommended for promotion, although others on the staff are perhaps more useful to the institution and more deserving than the party fortunate enough, for reasons above stated, to be close to the chief. What measures of relief for this condition would Doctor Johnston advise?

We know of one department where the staff, rebelling at the recommendation made by their head, drew up and submitted to their president counter resolutions recommending a fellow member other than the one favored by their chief. But for the president to give heed to such mutinous (?) expressions, when, as the Doctor shows, he is dependent upon the various heads, would be destructive to all system and discipline.

The author intimates that men of a department should be at liberty to discuss matters of their division or department freely with the dean of a college or even carry their criticisms and complaints to higher officials. Arguing by analogy, we must assume that he would have the heads do the same—namely go around their dean and lay their woes before a president or even, disregarding the latter official, go directly to the board of regents. We hardly believe that such a system or, rather, lack of system, was in the author's mind at the time he wrote the lines referred to. If so, on the principle of "What is sauce for the goose, etc.," it would appear only right that, if the dean of a college should consult the members of a department, disregarding the head, he should expect an equal disregard of professional etiquette on the part of a head.

We doubt also whether Doctor Johnston, when he states, referring to "the results of arbitrary power placed in the hands of single men without check or publicity" that "such a system always breeds dishonesty and crime," really refers to conditions in any *university;* if such is the case, he uses somewhat strong language. One might, at this point, be a bit facetious, and we are tempted to ask the Doctor what, in his opinion, the result would be if this arbitrary power were placed in the hands of married men?

Without, in any way, taking issue with the excellent article referred to, it suggests certain phases of the problem which Doctor Johnston did not discuss and which we mention here at the risk of being regarded presuming—believing that the subject is one which merits free expression from all standpoints.

This thought occurs to the writer. The head of a department is generally several years the senior of his men and with that seniority *should* go a maturity of judgment born of an experience generally lacking in the younger men. Further, allowing that all the undesirable traits listed by Doctor Johnston may exist in a head, are we not liable to find just as many or more undesirable characteristics in the numerous young minds under him with the additional factor that the younger minds of the staff have not reached that point in development where they can see the futility of such characteristics?

Again (and here is a weakness in some heads, fortunately of rare occurrence, which Doctor Johnston fails to take cognizance of), we sometimes see the head of a department seeking to climb into favor with a dean, president or board of trustees at the expense of another department or other departments, by depreciating the work of others, by ridiculing or criticizing suggestions not emanating from his own department, by intimations and even fabrications regarding the efficiency of an associated department, etc. Fortunately, this characteristic on the part of a head is rare, although the fact that it does exist in institutions is apparent to almost any worker who has been connected with universities for twenty years or more.

Now, then, admitting that this weakness exists in some heads and realizing that characteristics of this kind are found more or less in many men, must we not admit the possibility of the same existing in the minds and characters of one, two or several of the men under a head? Do we not see young men in the profession, desiring prominence and advancement in a department, impatient, selfishly critical of their chief, undermining his position when possible, with the hope of personal advancement and so jealous of their associates of equal rank as to resort to ridicule or fabrication at their expense, if it appears to them necessary for their personal ambition? This view, of course, is an extreme one—purposely so—that we may in taking to heart Doctor Johnston's excellent remarks, not fail to see the other side of the question.

We know young men to-day—men of pronounced mentality—hypercritical of their chief and insistent upon the merits of their own views of administration—views which they might radically change after attaining maturity of experience. Some of these young men are of such self-satisfied temperament that, in years to come, if they attain positions of authority to which they aspire, they will be more dictatorial in their departments and more hide-bound in their views than the chiefs whose views they now seek to belittle.

Perhaps enough has been said to indicate to us, using, in part, a time-worn phrase, that there may be a middle course and that, in swinging from the rocks on one side of the strait, care should be taken to prevent collision with the opposite shore.

The efficiency of a department is, of course, the standard by which it is judged in the upbuilding of a university. What better basis is there than its ability to give graduate work? We know of an institution fortunate in the possession of a dean of its graduate school who, with remarkable ability in this particular line, has, by many means, not necessarily through the heads, acquainted himself with the powers in this direction, exhibited by the various departments in his institution. His records are, therefore, an index of the comparative merits and demerits of different departments and will doubtless be used for reference when occasion demands. Does this not show a tendency to get away from the autocracy of the present system?

In the writer's own department, a most happy condition, recently inaugurated, prevails. The leading members of the staff are section heads, each with his own particular line of work, his own experimental projects, his own employees, his own budget. The responsibility of expenditures and results rests directly upon the section head and he is judged accordingly. The chief of the department has a general oversight over the work of his staff and is in charge of the executive work of the division. Regular meetings of the staff, with the chief of the division as chairman, are held for the purpose of transacting business pertinent to the division, such as the approval of projects or of publications, by vote; plans for the betterment of work, courses given in college, etc., etc., and it has been found that fostering a cooperative spirit upon the part of the men and emphasizing their individual responsibility gives far better results than the opposite policy—one which is still followed (sad to relate) by department heads who have not yet "seen the light."

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SCIENTIFIC BOOKS

Gruppenweise Artbildung. By HUGO DE VRIES, Professor of Botany in Amsterdam. Berlin, 1913. Verlag von Gebrüder Borntraeger. Pp. viii + 365. Figs. 121. Colored plates 22. Price 24 marks.

Somewhat more than ten years has passed since the appearance of the first volume of "Die Mutationstheorie" and we are most fortunate to have from Professor De Vries another book that is an extension of the former discussion and that also brings forward a remarkable body of new observations of very great interest to the workers in genetics. It is not often that an investigator is able to follow an earlier work of the scope of "Die Mutationstheorie" with a book of as great import, yet we have in "Gruppenweise Artbildung" a volume that perhaps outranks the first contribution in matter and in exposition. It will surprise even the students of *Œnothera* to note the remarkably wide range of crosses among these forms that De Vries has made and the extraordinary mass of observations that he has accumulated. These are carefully indexed and readily accessible for reference.

The first part deals with the origin of species by mutation. This is a summary of the views developed in "Die Mutationstheorie" with De Vries's answers to various criticisms that have been expressed to about the year 1912. There has been no essential change in his interpretation of the "mutations" from *Œnothera Lamarckiana,* an explanation of which is offered by a somewhat more detailed statement of his theory of intracellular pangenesis. Pangens are assumed in any individual to be in an active, inactive or labile state, and mutations arise when they are in the labile condition.

As to the status of *Œnothera Lamarckiana,* De Vries stands by his original position. It is to him representative of a wild species of American origin. Papers of the reviewer concerning the identity of Lamarck's plant of 1796 and on the problem of the origin of the *Lamarckiana* of De Vries's cultures have appeared too recently to find a place in his discussion, and probably for the same reason there is no discussion of the studies of Heribert-Nilsson. De Vries takes a clear position against the view that the "mutants" of *Lamarckiana* represent the splitting of a hybrid form. He believes that hybrids between species of *Œnothera* breed true as illustrated by his reported observations on the cross between *biennis* and *muricata.* When an *Œnothera* cross gives a wide range of variants in an F_2 generation, as in the case of the hybrids between *grandiflora* and American types of *biennis,* De Vries apparently assumes that a mutating habit has descended from one or both of the parents.

Part two, treating of reciprocal and double reciprocal crosses, gives in detail the data upon which the conclusions reported by De Vries in 1911 were based. *Œnothera biennis* and *O. muricata* of Holland are two well-defined species, readily distinguishable, which breed true. Their reciprocal hybrids exhibit constant and marked differences and in the most striking of their vegetative characters strongly resemble the pollen parent. So uniform is this behavior that De Vries expresses the results with respect to the characters concerned by two formulæ—$b \times m = m$, and $m \times b = b$. The important peculiarity of these hybrids is then the fact that they differ sharply from one another to a degree very unusual in reciprocal crosses. Furthermore, the reciprocals are reported to breed true without exhibiting variation in the F_2 generation as might be expected.

These reciprocal crosses may be crossed with one another in two ways to give double reciprocals—(*biennis* × *muricata*)×(*muricata* × *biennis*), and (*muricata* × *biennis*)×(*biennis* × *muricata*). When this is done the contrasted characters of the parent type which occupies the center of the formula appear to drop out and the resulting double reciprocal hybrid presents the characters of the parent which occupies the peripheral position. Expressed in simple formulæ, which only apply to the vegetative characters under consideration—$(b \times m)\times(m \times b) = b$, and $(m \times b)\times(b \times m) = m$. The products of the double

reciprocal crosses, like the reciprocals, also breed true.

Modifications of the double reciprocal crosses, termed by De Vries sesquireciprocals, may be made by combinations of the reciprocals with the parents in such a manner that similar parent types occupy the periphery of the formulæ—$b \times (m \times b) = b$, $(b \times m) \times b = b$, $m \times (b \times m) = m$, and $(m \times b) \times m = m$. Other arrangements with the peripheral positions of the formulæ occupied by different parent types give iterative hybrids—$b \times (b \times m) = m$, $(m \times b) \times b = b$, $m \times (m \times b) = b$, and $(b \times m) \times m = m$.

The explanation of this remarkable behavior is not as yet determined. An attractive hypothesis postulates the differentiation of classes of gametes carrying the characters of the parents in pure form and their appropriate combinations either by selective fertilization or through the elimination of such gametes as do not fit into the assumed schemes of combination. But these and other speculations must await the results of cytological studies as well as further experimentation. An interesting peculiarity of these crosses is their very high degree of sterility and it remains to be seen whether the same phenomena will be found in other *Œnothera* species crosses that are more fertile. Thus it is possible that numerous and varied types of gametes may be developed by the hybrids, as theoretically would be expected, but that physiological conditions will allow only certain types to mature or function.

A long and detailed account of twin hybrids constitutes the third part. These classes of hybrids were first discovered by De Vries among hybrids of *Lamarckiana* with other species of *Œnothera*. He has since greatly extended his observations and finds twin hybrids also differentiated when certain "mutants" from *Lamarckiana* are similarly crossed (*e. g.*, *nanella, lata, scintillans, oblonga* and also *lævifolia*). *Brevistylis* in such crosses follows a Mendelian ratio and *gigas* gives intermediate hybrids. The twin hybrids appear in the first hybrid generation which consists of two sharply contrasted groups. In one group, termed *læta,* the characters of *Lamarckiana* are strongly dominant over those of the other parent. In the other group, termed *velutina,* the characters of the other parent dominate those of *Lamarckiana.* The proportions of the *læta* and *velutina* types appear to vary greatly in different cultures. The *velutina* types breed true in the second and later generations, but the *læta* forms were found in certain cultures to split off new lines of *velutina* in successive generations.

Another pair of twin hybrids, very different from *læta* and *velutina,* are distinguished as *densa* and *laxa.* They appear in crosses between certain broad-leaved forms of American *biennis* and *cruciata* with *Lamarckiana* or its "mutants." The distinctions here concern chiefly the form of the foliage and the number of capsules over a given length of stem; *densa* is broad-leaved with thickly crowded capsules, *laxa* has smaller leaves and capsules less numerous. The *densa* type breeds true, the *laxa* throws off in successive generations still another form, *atra,* distinguished by dark green leaves.

With the twin hybrids may be found a class of delicate and dwarfed plants, the seedlings of which are etiolated. These are named *gracilis* and they are present in very diverse proportions. Very many of the *gracilis* types die as seedlings, a few with care may be brought to maturity as narrow-leaved plants mostly sterile. The classes of dwarfs that the writer has reported for a number of crosses between *grandiflora* and American *biennis* may correspond to this group. Finally among the twin hybrids are occasionally found plants of marked size or luxuriance which appear to hold to the cultures as a whole a relation somewhat similar to that of *gigas* or *semi-gigas* to *Lamarckiana.* This type is called *hero.*

De Vries offers an explanation of twin hybrids, and of the *gracilis* and *hero* types through his theory of mutation and intracellular pangenesis. Mendelians may attempt to find in the phenomena the results of recombinations of multiple factors, although they will have difficulties in working out consistent

ratios. Others may be satisfied with an elastic view that allows of profound interactions of factors upon one another with their material modification in the "melting pot of cross breeding."

The lengthy fourth part is an examination of the chief new species of De Vries's cultures as to their behavior in crosses, with special reference to an explanation of this behavior on the theory of intracellular pangenesis. An immense amount of detail is presented, well sifted, however, by the summary and conclusions. It is interesting to note that of these new species *gigas* alone is considered as progressive; *brevistylis, rubrinervis* and *nanella* are regarded as retrogressive, *lata* and *scintillans* as degressive, and *oblonga* as anomalous.

Finally a fifth part on the cause of mutation gives us the latest statement of De Vries's position. This part consists of discussions of a number of topics related to other portions of the book or to earlier publications of the author, and constitutes a general summary. "Gruppenweise Artbildung" results from a gradual accumulation of mutations on the part of a species, and hybridization to De Vries includes a very much wider range of phenomena than the types interpreted by Mendelian analysis.

A comprehensive bibliography of *Œnothera* experimental literature, a full and very valuable citation of the crosses that De Vries has personally made among the Œnotheras, and an excellent index complete the volume. The 121 text-figures throughout the book are of an unusually high grade, and the 22 colored plates admirably executed. It is greatly to be hoped that the author and publisher will promptly arrange for an English translation.

BRADLEY M. DAVIS

Monographia Uredinearum seu specierum omnium ad hunc usque diem cognitarum descriptio et adumbratio systematica. By P. and H. SYDOW. Volumen III., Fasciculus I.: Pucciniaceæ, cum 7 tabulis. Lipsiæ, Fratres Borntræger. 1912. 8vo. Pp. 1–192.

The appearance of the first part of the third volume of the "Monographia Uredinearum" by P. and H. Sydow has been of especial interest to mycologists because it has given the first bit of information concerning the classification which the authors are following, or propose to follow, in this work. The two earlier volumes (Vol. I., Genus *Puccinia,* 1902–4; Vol. II., Genus *Uromyces,* 1910) were entirely taken up with the treatment of two genera, *Puccinia* and *Uromyces,* without the slightest hint as to how they were to fit into any general arrangement. It seemed evident from the beginning that these two genera were given preferences on account of their size and popular importance and not because they might appear in that order in any scheme of classification. The correctness of this surmise is now well shown. The third volume is begun with a key to the genera of the family Pucciniaceæ, a total of twenty-five being recognized. In this key *Uromyces* is number 8 and *Puccinia* number 10. In the preparation of a work of this nature there are many obvious advantages in not being hampered by the publication of a key at the start, before all of the genera are fully studied, which must thereafter serve as a guide. The freedom with which these authors began their task they have deliberately relinquished, for they are herewith publishing a key to twenty-five genera although the descriptive accounts to date only cover fully the first sixteen of them.

To do the monographic work first and follow it with a key will, however, evidently not succeed in eliminating difficulties, as is evidenced by an examination of the present part. For example, one finds that the genera *Uropyxis* and *Diorchidium* are recognized in the key as valid, although they have been treated already in previous parts as synonymous with *Puccinia.* With the exception of these four genera, which have been treated previously, the present part takes up the genera in the order of the key and proceeds as far as the generic description of *Uromycladium,* which is the sixteenth genus.

The genera in the order of their appearance are as follows, *Gymnosporangium, Hamasopra,*

Gymnoconia, Phragmidium, Phragmopyxis, Blastospora, Rostrupia, Triphragmium, Hapalophragmium, Sphærophragmium, Anthomyces, Uromycladium. These are distributed among the three subfamilies, Phragmidieæ, Uropyxideæ and Puccinieæ, into which the family is divided. The limitation placed on these subfamilies has not been very rigid, for the genus *Triphragmium* is included in two of them, the Phragmidieæ and the Puccinieæ. The authors, however, state that they are uncertain regarding the place which *Triphragmium, Hapalophragmium* and *Sphærophragmium* should occupy in their key. The remaining genera of the family Pucciniaceæ are *Dicheirinia, Gerwasia, Hemilea, Ravenelia, Neoravenelia, Kuehneola, Pucciniostele, Skierka,* and presumably we may expect the next part to deal with them in the order given.

The classification shows conservatism on every hand and especially in the selection of the characters upon which it is founded. The old idea of the importance of the teleutospore is maintained. Such a method can be made to work very well as long as only the common things are considered from a "practical standpoint," but when all forms are considered from a scientific standpoint it can not be said to have much in its favor. The result in the present key is uncertainty and lack of uniformity. The attempt to arrange the key in such a way as to show relationships of the genera is highly desirable, but is not attained when the boundaries for genera are so loose that species contained within them admittedly indicate relationships to different subfamilies. But the composite character of genera can not be avoided with the one character scheme as a basis for grouping. Neither can the segregation of closely related forms be prevented as long as this system is maintained. The purely artificial character of number of cells in the teleutospores will throw forms which are of undoubted relation into different genera. Numerous examples illustrative of this feature have already been cited in the literature[1] and more are constantly being found as careful comparisons are made. An arrangement which places the peach and plum rusts in the genus with *Puccinia graminis* and then separates a few forms from *Puccinia* into the genus *Rostrupia* may be "practical," but if it is any way natural it must be accidental. If *Rostrupia* which differs from *Puccinia* only in having the teleutospores with 3 or 4 cells should be maintained, it is not clear why several of the species of *Gymnosporangium* which have more than 2 cells should not be separated into a genus by themselves, or why the old genus *Phragmidium* should not be broken up into several genera, since the number of cells in the teleutospores in this group is highly variable. If the forms on Rosaceous hosts which have 2–several cells are worthy of generic standing outside of the ordinary *Puccinia* and *Rostrupia* genera, then it is not clear why the 1-celled forms on these hosts should not be separated from *Uromyces,* but such has not been done.

The bulk of the part is taken up by the genera *Gymnosporangium* and *Phragmidium,* 136 out of 192 pages being devoted to them and divided nearly equally between them. The monograph of *Gymnosporangium* is of interest in comparison with the one published by the writer about a year earlier as a Bulletin of the New York Botanical Garden.[2] The order of arrangement of the species, the plan of the keys, and the form of the descriptive accounts are the same as introduced in the writer's bulletin. Three additional species are included in the Sydow monograph, all described since the appearance of the writer's publication and founded on material not seen by him. As to the validity and relationship of the species there has not been the slightest disagreement. With one or two exceptions it is also to be noted that the specific treatment of the *Phragmidium* group is identical, so far as North American species are concerned, with Arthur's account in the Uredinales, "North American Flora." The latter, however, refers some of the species to other genera, *Earlea, Kuehneola,* while the Sydows refer all to the genus *Phragmidium.*

As regards certain nomenclatorial questions,

[1] Arthur, *Mycologia,* 4: 54–56, 1912, and Orton, *Mycologia,* 4: 194–204, 1912.

[2] Vol. 7, No. 26, 1911, pp. 391–483.

it is to be regretted that no such agreement of opinion can be recorded. When a full list of synonyms is included, as in the monograph, no great difficulty is likely to be experienced even when different authors choose to select different names as the one to be maintained, and yet instances will arise which are deplorable. The very first species in this third volume of the Sydow monograph raises some questions. They reject for it the writer's combination, *Gymnosporangium Blasdaleanum* (Diet. & Holw.) Kern, although that specific name is without question the oldest, presumably because it was founded on an æcidial stage, *Æcidium Blasdaleanum* Diet. & Holw. They also refuse to admit for this species another combination of the writer, *G. Libocedri* (P. Henn.) Kern, although this is founded on the "all important" teleutosporic stage and is the oldest specific name thus applied. Henning[3] used the combination *Phragmidium Libocedri,* cited his specimen in full, and accompanied it with an adequate description. He was in error in suggesting that *Gymnosporangium Libocedri* Mayer was the same as his plant, and we can not say that he transferred the Mayer name to *Phragmidium,* but he nevertheless very evidently did intend to apply the name *Phragmidium Libocedri* to his plant and he characterized it accordingly. To reject this specific name because he did not specially propose it *as new* seems to be a motive which is contestable and of little import. Such a procedure not only seems illogical, and is not only not followed by most botanists, but in many similar cases not by the Sydows themselves. They accept *G. Sorbi* Kern, *G. Harknessianum* Kern, *G. Photiniæ* Kern, *G. hyalinum* Kern, *G. tubulatum* Kern, *G. transformans* Kern and *G. bermudianum* Earle, although not one of these was proposed, as a new name in the genus *Gymnosporangium!* They were all transfers of æcidial names as is evidenced by the inclusion of the original authors' names in parenthesis or the words *comb. nov.* Why not say that in these instances the establishment of names has not been accomplished if one refuses to recognize æcidial names and there has been a failure to propose them in *Gymnosporangium* as new? In the case of *G. Amelanchieris* Ed. Fisch. the matter is somewhat different, for although *Amelanchieris* is a name proposed by de Candolle for the æcidial stage of the plant to which it is now applied, Fischer distinctly stated that he was not transferring de Candolle's name, but proposing another just like it as new. This appears to satisfy the conditions which it seems that the Sydows would like to impose and yet in this very instance they have given evidence that they did not regard Fischer's name as a new one but as a transfer, for their Uredineen No. 2287 was issued as *Gymnosporangium Amelanchieris* (DC.) Ed. Fisch., with *Æcidium Amelanchieris* DC. given in parenthesis as a synonym. To go back again to the first species it is to be noted that the authors after rejecting the specific names *Blasdaleanum* and *Libocedri* for it see fit to retain their own name *aurantiacum,* although there is a *Gymnosporangium aurantiacum* of Chevallier[4] published in 1826, seventy-eight years earlier.

As regards the standing of names applied to stages other than the teleutosporic, it seems evident now that these authors will accept them in cases where there is no teleutosporic name. A number of instances have already been cited in a foregoing paragraph. However, in previous parts it is not clear that they have been willing to do this; for example *Uromyces Silphii* Arth. founded on a specific name, which was originally applied to an æcidial stage, has been renamed *Uromyces Junci-tenuis* Syd. nov. nom.[5]

As already suggested, unusually full and accurate lists of synonyms have been included, but the arrangement of these is not always uniform or of such a nature as to make them most usable. It is not a straight chronological arrangement such as used by many authors, but the names are grouped according to the genera so that specific names belonging to the same genera come together. If the order of the

[3] *Hedwigia,* 37: 271–72, 1898.

[4] *Fl. Env. Paris,* 1: 424, 1826.

[5] See Sydow Monog. Ured., Vol. II., fasc. II., p. 289, 1910.

names under a genus were chronological it would facilitate matters, and while it seems to be thus in many cases, it is by no means uniformly so. Under *Gymnosporangium clavariæforme* four synonyms in the genus *Æcidium* are cited, dated as follows, 1801, 1808, 1801 and 1905; four are also given here under *Roestelia* in this order, 1849, 1887, 1880, 1815.

In the matter of illustrations the present part shows a considerable improvement over the preceding parts. The drawings of the spores show more accuracy in preparation and do not look so diagrammatical. The fact that other structures aside from the teleutospores, such as peridial cells, have had representation in these illustrations is a matter worthy of favorable comment. The printing of the plates on the regular paper makes them somewhat difficult to find. Since not all species are illustrated it is not always possible to tell from the plate and figure number in which direction from the description one should turn to find the illustration. This could be avoided by including the page number of the plate (they all have page numbers although they are not printed upon them) along with the plate and figure number where the reference is given at the end of a description. It is also very difficult to find the description of a figure if one sees an illustration and desires to look it up. Aside from the figure number there might also be given the number of the page where the description occurs. These items would increase the amount of labor in preparation, but would enhance the value of the work sufficiently to warrant it.

The authors are to be praised for the great amount of valuable work they are doing with this difficult group of fungi, and mycologists in general must be exceedingly glad that the preparation of this large monograph has proceeded so steadily. With the appearance of the present part the larger and more important genera have received treatment. The world-wide treatment of such complex plants must necessarily entail an enormous amount of labor and must necessarily involve the inclusion of forms concerning which first-hand information may be meager. These authors must be commended for the use which they make of the work of other specialists.

That they have drawn freely upon the observations of others is especially apparent in the arrangement of the keys, the form of descriptive accounts, the synonymy, and in the preparation of illustrations in this third volume. A deplorable feature is that the works of other writers and investigators may receive only slight or even no credit for the parts which are adopted by the authors or followed closely, whereas in minor portions, where there may be a difference of opinion they see fit to call attention to them in such a way as often to bring discredit upon the works which are really so largely utilized.

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The Primitive Family as an Educational Agency. By JAMES ARTHUR TODD. G. P. Putnam's Sons, New York and London, 1913.

"The Primitive Family as an Educational Agency" is frankly a brief against "the family superstition" in education, a brief, let one in turn be frank enough to say, that is hardly needed by the ethnologist and that will not be heeded, I venture to predict, by the sentimentalist.

To him or her Professor Todd has undertaken to show that the past of the family is not all it is supposed to be, that monogamy, for example, is an acquired predilection, that in primitive circles kinship may be an uncertain notion and that the "natural bond" between parent and child is merely a latter day figment.

So sympathetic am I with Professor Todd's main undertaking, the cornering of the sentimentalist, and so much in agreement with his general contention that non-familial agencies may have been or may become much more efficient in education than the family that I am reluctant to criticize his method and regret having to question several of his minor arguments.

As to his method, it may be enough to merely describe it as the method of illustra-

tion, a method apt to be as satisfactory to those with you as it is unconvincing to those against you. Professor Steinmetz has pointed out for all time that haphazard instances are not ethnological proof. The soundness of the comparative method rests on the scrupulous raking through of ethnographic data. Data assembled uncritically may also be misleading. Some of Professor Todd's illustrations are misleading. In citing modern Mexicans (p. 42), for example, he is not citing, as the text would convey, a primitive people. On pp. 39 ff. he confuses the grounds of divorce with the extent of it. In referring to the *pirravru* relationship of the Australian Blackfellow (p. 35) as an affair of intermittent promiscuity he appears ignorant of the fact that it is quite as stable a relationship and as carefully regulated as the coexistent pairing relationship.

Fortunately in spite of this misconception Professor Todd has not fallen into the old promiscuity pitfall. Still he argues that in view of well verified sporadic cases of group marriage and of periodic license monogamy is not an innate instinct. To this contention it may be said that group marriage is still too obscure a fact to be called upon with much assurance in argument. Whether periodic sex license may not be more adequately explained as a phenomenon of the breaking down of habit than of the persistence of an old habit is certainly an open question. As for other deviations from monogamy, polygyny, polyandry, and prostitution, are they not due to social processes which have rendered monogamy inadequate, to considerations of social prestige or dominion, to wealth or poverty, to ancestor worship? More social processes are involved in polygamy than in monogamy.

In education through the family does the form of marriage matter at any rate quite as much as Professor Todd together with his sentimental opponents would have us think? Until very recently purposive education in or out of the family has invariably taken the form of discipline, and in the polygynous patriarchal family there has ever been a greater degree of discipline for offspring than in any monogamous type of family. Then too has not even brittleness in marriage been exaggerated as a pernicious effect upon offspring? Domestic education is essentially a matter of imitation, and one adult may be imitated as well as another. Family discord is of course pernicious, but brittleness in the marriage tie is likely to preclude discord, if anything.

But discipline is not education, I shall be told, nor is imitation. True, not in the modern meaning of the term education; but is that meaning to be reckoned with in considering any type of familial education as yet known? In all kinds of primitive education and in all kinds of familial education, primitive and modern, there has been but one purpose, the producing of conformity to type. If Professor Todd had taken this thesis as a basis for his arraignment of the rôle of the family in education, not merely pointing to it from time to time (pp. 146, 171), he would have been on safer and, I may say, more fertile ground. Moreover if he had stuck to the proposition he himself laid down at the outset that the bond between husband and wife or between parent and child is a primeval tropism based on the satisfactions resulting from safety and pleasure contacts he would have been under no necessity to show that monogamy was not an instinct, or that in view of the practise of adoption there was no natural bond in parenthood. Habitual association is the natural bond in parenthood whether adoptive or not. It is also the natural bond in marriage, whether brittle or lax. The pull of habit, whether in parentage or in marriage, Professor Todd together with many other students has overlooked.

Had he allowed for it, he would have escaped making several false generalizations. He would not have said that any sort of sex conduct was allowable among primitive men provided it did not infringe on the rights of others (p. 35)—unless of course he included among rights the right not to be discomfited by innovation, a right most jealously safeguarded by primitive man. He would not have argued that the family in which indefinite notions of kinship existed could not have exerted any great disciplinary force (p. 86).

(How about the coexistence of juridical parenthood and of discipline in the patriarchal family?) Nor would he have concluded that sex taboos, the demarcation of masculine and feminine interests, resulted in social discord (p. 54). He would have realized that sex taboos have quite the opposite effect, protecting the habits of one sex against the habits of the other. He might also have realized that age class similarly protects itself against age class and that respect for age is merely a survival of the rigid age class demarcations of primitive circles, in no sense a development (p. 131).

Until comparatively recently, "the formation of a body of habits," sex habits, age-class habits, family, clan or tribe habits was the goal of all education. As Professor Todd has well pointed out (pp. 143–4), primitive education planned to adjust youth to a static environment, to fit each boy and girl into a set place from whence no departure was possible, except into another set place. Modern education at its best plans to develop in all of us adjustability to a changing environment, together with a capacity to control our environment, *i. e.,* it plans to develop personality. To its part in this new venture of education the modern family is not yet awake. Hence its discredit in the eyes of Professor Todd and other modern educators. Once it realizes that of all educational agencies it has unique opportunities to develop personality, that far better than the school or the club it may lead a child to think for himself and to have the courage of a minority, once the family becomes alive to this new rôle—perhaps the coming "transcendent and valuable" rôle Professor Todd has in mind for it, it may assert with success its old claim to educational prestige—and not before.

Elsie Clews Parsons

NOTES ON THE SEA ELEPHANT (MIROUNGA LEONINA)

Mr. Robert Cushman Murphy has published in the *American Museum Bulletin*[1] an interesting and splendidly illustrated paper entitled "Notes on the Sea Elephant *Mirounga leonina*" (Linné). This article embodies the observations made by Mr. Murphy during a whaling and sealing voyage to the South Georgia Islands on the brig *Daisy* of New Bedford. Although sea elephants have been hunted for many years and thousands have been killed for commercial purposes, but little accurate information as to their life history is to be found in the literature of the species. This is perhaps partially due to the fact that their habitat lies on the desolate, storm-swept islands of the South Atlantic, in a region which holds out few inducements to the traveler and that almost the only visitors to their uninviting breeding grounds were those who came to slaughter the animals for commercial gain.

Too few of these hunters were interested in anything but the number of gallons of oil which could be tried out from each carcass, and ship after ship returned loaded to the gunwales with oil but empty of information concerning the habits of the greatest of all the seals which they were sweeping from off the earth. This relentless slaughter has long since passed the bounds of safety and the sea elephant bids fair to soon be numbered with the Steller's sea cow, an animal which has been swept away, leaving little but traditions behind.

Mr. Murphy's notes, taken with the care and interest of one who came to study and not to kill, are thus especially interesting, and combined with his splendid photographs form a valuable contribution to the life history of the elephant seal.

Dr. C. H. Townsend's rediscovery of the northern sea elephant (*Mirounga augustirostris*) on Guadaloupe Island, and the valuable collection of specimens and photographs which he secured, have done much to elucidate the life history of that species and Mr. Murphy has well supplemented his work by this study of the southern animal.

Roy Chapman Andrews

American Museum of Natural History

[1] *Bull. Am. Mus. Nat. Hist.,* Vol. XXXIII., Art. II., pp. 63–79, pls. I.–VII.

SPECIAL ARTICLES

CARDS AS PSYCHOLOGICAL APPARATUS

"THREE thousand dollars a year for a good instructor and one dollar for paper and pins" are considered a sufficient equipment for a fair elementary psychological laboratory by at least two distinguished American psychologists. The present paper is a brief account of some of the uses that may be made of paper in one form only, playing-cards, picture post-cards and library cards as found on the market. The uses described here relate to former work, to work now being done and to further possibilities.

In general, cards commend themselves as suitable experimental material on account of their cheapness, accessibility, generality, more or less familiarity and their standard nature in shape, size, weight and quality. The standard nature of the playing cards needs no qualification, and the variations of picture post-cards in shape, form, design, etc., are easily described and their pictorial features readily reproduced when necessary.

The frictionless or slipping quality of playing cards commends them for work requiring speed; they were first used in this connection by Jastrow[1] to determine the so-called discrimination (distinction) and choice time. And as a class demonstration of the time relations of these two mental processes the cards remain perhaps as simple and as cogent as any material now in use. Seven years later Professor Bergstrom[2] used "unprinted cards with the best slipping qualities" to study the resistance or interference offered by an old habit to the acquisition of a somewhat related one. He took advantage of the diminishing states of resistance to measure the rate of forgetting a habit. And further study has shown that an established habit may not simply interfere, but that it may also favor the acquisition of a new habit, *i. e.*, an old habit may blow both *cold* and *hot*, as it were, in its effect upon a learning process, and thus a study of the potency of one learning process upon another is made possible by the use of cards. Coover and Angell[3] determined the effect that skill in tossing colored cards into six small compartments bearing colored labels has upon the rate of manipulating the typewriter. In such experimentation playing-cards have proved exceptionally useful[4] for the reason that the processes involved in their use are susceptible of analysis to the unitary stage. So that it becomes possible to make a quantitative estimate of the units of transference and interference.

The study of the "Learning Process" begun in this country by the original work of Bryan and Harter[5] on "learning telegraphic language" inspired psychologists to draft the instruments of both work and play into the service of experimentation. The hand-ball and short-hand,[6] the game of chess[7] and the typewriter[8] have each in turn made notable contributions to the learning process. But it is evident that neither these instruments nor their uses are adaptable to the laboratory as class apparatus. Economy of time alone forbids. Hand-balls are inexpensive, but ball-tossing as a learning process is narrow in its range and the operation too fantastic for laboratory purposes. There is need of simple ways and means whereby individuals of large classes may participate in the experimental operations. Among the more successful means now in use are nonsense syllables[9] and the principle of reciprocal substitution of letters, figures and conventional symbols devised by Jastrow. We would add to the list the use of playing-cards *in connection with a distribution-case.* The case and its uses require a brief description.

We have found that a case 18 inches high, 36 inches long and 4 inches deep will furnish space for 54 compartments, having six in the vertical dimension and nine in the horizontal.

[1] SCIENCE, Vol. VIII., 1886.

[2] *Amer. Jour. Psy.*, Vol. 5, 1893.

[3] *Amer. Jour. Psy.*, Vol. 18, 1907.

[4] Kline and Owens, *Psych. Rev.*, Vol. 20, 1913.

[5] *Psych. Rev.*, Vol. 4, 1897.

[6] Swift, *Amer. Jour. Psy.*, Vol. 14, 1903.

[7] Cleveland, *Amer. Jour. Psy.*, Vol. 18, 1906.

[8] Book, Wm. F., Univ. of Mont. Studies, 1908.

[9] Ebbinghaus, "Ueber das Gedachtniss," 1885.

This arrangement furnishes a compartment for each card of the pack of fifty-two, and at the same time preserves an approach to equality between the dimensions without a large excess of compartments. Each compartment is three and a half inches in length by two and three fourths inches in height, and admits completely the average playing-card when *tossed.* Each compartment is provided with a metal clip for holding detachable labels cut from the cards.

Even one unacquainted with card games and card lore will realize upon slight reflection the well-nigh endless variety of combinations made possible by their qualities of form, color and number. The case is so labeled that the opportunity for forming associations between contiguous boxes and two or more successive distributions is rare. The cards may be unstacked or stacked, according to purpose. If the latter then learning the order of the cards affords a method for the study of serial learning. The compartments receiving the cards for any series are viewed only during the process of distribution, thereby creating conditions for the study of the sense of position. Perhaps we shall make better progress in suggesting the possibilities of problems and methods by briefly submitting a problem.

Let *A* distribute the pack according to number and color, throwing to the *diamond* and *club* compartments only. This requires 26 compartments, 13 of which receive the like numbered red cards and 13 the like numbered black cards. Let *B* distribute the pack to 26 compartments throwing to the red labels according to the following plan: Throw spades to hearts, and clubs to diamonds of the same number; *e. g.*, K and Q of Clubs would go to K and Q of diamonds, respectively, while the hearts and diamonds would be distributed to their respective compartments. We say they are "resident." *A* and *B* now practise for an equal time under uniform conditions until they can distribute the pack of 52 in about 55 seconds. They, then, exchange work and although the cards are stacked the same for both series, *A* manifestly will have to learn the location of 13 heart boxes and *B* 13 club boxes; this in itself is a trifle, but it is coupled with the fact that the *directions* of the movements from box to box have made for each subject about 48, out of 52 possible, changes—a high percentage of motor interference. A further study of the consequences of the exchange of work shows that 25 per cent. of the sensory processes of the new work is identical with that of the old, while 75 per cent. is different and causes interference.[10] If now the distributing practise be continued until the former speed is attained ample opportunity is given to study the operation and fate of transferable and interferable motor and sensory processes, respectively. And of course material is furnished for two "learning curves," one for the first and one for the second series. Doubtless several other problems solvable with this material and methods will occur to the psychologist. In this connection it may be stated that it has been demonstrated that the playing cards and the case constitute a psychotechnical instrument for scientific study of certain industrial operations. This has already been demonstrated by their application to the psychological problems involved in the distribution of mail to letter cases as done by post-office and railway mail clerks.

Picture Postcards.—The value of pictures for *aussage,* memory and imaginative tests is now generally recognized together with possibilities for the study of the more intricate problems of feelings and attitudes. The technique and methods in these latter problems have not kept pace with those devised for the study of sensations and the will. The more complacent methods of introspection are to no purpose in the study of feelings and emotions since they do not come to order in the laboratory nor wait for introspective analysis; and the physiographic methods hitherto employed require considerable supplementing before the nature and relationships of the feeling consciousness are fully understood.

Some attempt has been made to use the reproductions of classic paintings in the study

[10] For the basis and methods by which these quantities are determined see Kline and Owens, *Psych. Rev.*, Vol. 20, p. 224, 1913.

of feeling-tones only. And while such pictures are easily available their application is limited, for they are usually regarded by observers as pleasant or indifferent, seldom distinctly unpleasant. Their use is thus confined almost wholly in one direction, viz., that of esthetics. It is quite desirable to secure material capable of stimulating a wide range of feelings if we would make appreciable progress in their study. It appears that the advent of the picture postcards with their standard size, well-nigh endless variety and low price have more than supplied the experimental deficiencies of the classic pictures. The picture postcards make an appeal to the whole gamut of human affections. The technique for experimental purposes consists in selecting, adapting and in manipulating the cards so as to bring specific feelings into relief. To indicate uses as well as difficulties a few examples are submitted. The emotions that may be produced under laboratory conditions will always be rather feeble and so difficult to describe. The difficulty may be partially overcome by the use of picture postcards as material, some appropriate device for exposure and the law of dissociation as a method. According to James[11] the law of dissociation by varying concomitants holds for feelings as well as for sensations. The law states:

> What is associated now with one thing and now with another tends to become dissociated from either, and to grow into an object of abstract contemplation.

By alternating one picture with various others it is possible to bring to notice obscure feeling responses that would otherwise go unreported, *e. g.*, if a picture of children at play is alternated with that of a beautiful woman; it is often hard for an observer to say anything further than that the pictures seem to go well together. But if the picture of a drunkard be substituted for that of the woman, not only does the disgust at the new combination serve for an interesting study, but the former feelings can now be more readily described. We are thus furnished with a key to discover which feelings inhibit each other, which reinforce each other by contrast, and which fuse into one of a more general attitude. In short we are on the road to an analysis and synthesis of feelings.

The feelings aroused by the senses that respond to the stimuli of the outer or external world are usually objectified, *i. e.*, referred to the source of stimulation. For this reason observers are often at an utter loss to give an account of their attitude or to describe their feelings in response to a picture.[12] The psychologist's only refuge here is to call for repeated descriptions of the picture and to interpret the description in psychological terms. It is not difficult to devise conditions for readily repeating observations of the cards, and thereby enable the observer to carry the description a little farther each time. These descriptions, when carefully made, not only reveal the observers' feelings and attitudes but demonstrate the way in which apperception depends upon attitudes. These studies with the picture postcards have a practical bearing upon certain problems such as the order in which pictures should be hung in galleries, and the proper sequence and time exposure of lantern slides in illustrated lectures.

LINUS W. KLINE,
CHESTER E. KELLOGG

11 "Psychology, Briefer Course," p. 251.

12 G. Santayana, "The Sense of Beauty."

SOCIETIES AND ACADEMIES

THE ILLINOIS ACADEMY OF SCIENCE

THE seventh annual meeting of the Illinois Academy of Science was held in the engineering building of the Northwestern University, at Evanston, February 19 and 20, 1914, under the presidency of Frank W. DeWolf, director of the State Geological Survey. At the Friday session the following addresses were given:

"Recent Investigations of the Mineral Resources of the Country," by the president.

"Earth Tides," by Professor A. A. Michelson.

"The International Phytogeographical Excursion," by Professor H. C. Cowles.

"Recent Theories of Fertilization and Parthenogenesis," by Professor F. R. Lillie.

At 6:30 the members of the academy were entertained at dinner by Northwestern University. This was followed by a reception in the physical laboratory given by the local chapter of Sigma Xi.

At the Saturday meeting the following papers were presented by members of the academy:

"A Unified Science Course for High Schools," by Harold B. Shinn.

"Agricultural Science in the High Schools of Illinois," by A. W. Nolan.

"Reaction of Fishes to Temperature," by W. M. Wells.

"Soil Moisture and Plant Succession," by G. D. Fuller.

"The Vacuum Arc in Spectroscopy," by G. V. McCauley.

"Postglacial Biota of Glacial Lake Chicago," by F. C. Baker.

"Behavior Agreement Among the Animals of a Community," by V. E. Shelford.

"Evaporation and Soil Moisture in Forests and Cultivated Fields," by J. F. Groves.

"On Conditions Under Which the Vegetal Matter of the Coal Beds of Illinois Accumulated," by T. E. Savage.

"Comparative Analysis of Text-books of Zoology," by E. R. Downing.

"Recent Views Concerning Electrical Conduction in Solutions," by L. I. Shaw.

"Preliminary Note on the Cyclonic Distribution of Weather Elements for Davenport, Iowa," by A. D. Udden.

"Water Control at Evanston," by W. Lee Lewis.

At the business meeting it was decided to hold the next meeting at Springfield, February 18 and 19, 1915. The officers for the ensuing year are: *President*, Dr. A. R. Crook, Director State Museum, Springfield; *Vice-president*, Professor U. S. Grant, Northwestern University, Evanston; *Secretary*, Dr. E. N. Transeau, Eastern State Normal School, Charleston; *Treasurer*, Professor J. C. Hessler, Millikin University, Decatur.

Edgar N. Transeau,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 519th meeting was held in the assembly hall of the Cosmos Club, January 10, 1914, with President Paul Bartsch in the chair. Five new members were elected. The discussion on parallel development was continued. L. Stejneger spoke on parallelism as exhibited in reptiles, while Barton W. Evermann and Theodore Gill discussed it as related to fishes. Messrs. Eastman, Bartsch, A. D. Hopkins and William Palmer also took part in the discussion.

The 520th meeting was held January 24, 1914, with President Bartsch in the chair. Five persons were elected to membership. The program consisted of three communications:

"Winter Bird-life in the Swamps of Alabama," by E. G. Holt.

"Pollen Protection in the Flowers of *Acacia* and *Anona*," by W. E. Stafford.

"The Problem of the Gliding Gull," by William Palmer.

The 521st meeting was held February 7, 1914, President Bartsch in the chair. One new member was elected. Two communications were presented:

"Notes on the Fossil Mammals of the Fort Union," by J. W. Gidley.

"Certain Seeds Used for Ornamental Purposes in the West Indies," by J. N. Rose.

The 522d meeting was held February 21, 1914, Vice-president J. N. Rose in the chair.

The program consisted of three communications:

"Seasonal Movements of Fishes at Lake Maxinkuckee," by Barton W. Evermann.

"An American Swastika," by Henry Talbott.

"Surface Temperature in the Humboldt Current and its Coastal Eddies," by R. E. Coker.

The 523d meeting was held March 7, 1914, with Vice-president A. D. Hopkins in the chair. Three persons were elected to membership. The program consisted of two communications:

"Remains of a Prehistoric Feast," by William Palmer.

"Further Evidence of Mutation in *Œnothera*" (illustrated with lantern slides), by H. H. Bartlett.[1]

The 524th meeting was held March 21, 1914, with President Bartsch in the chair. Two new members were elected. Two communications were presented:

"Arabic Interpretations of the Songs of Birds," by Paul B. Popenoe.

"Bird Migration in the Mackenzie Valley" (illustrated with lantern slides), by Wells W. Cooke.

D. E. Lantz,
Recording Secretary

[1] To be published in *Journal of Agricultural Research*.